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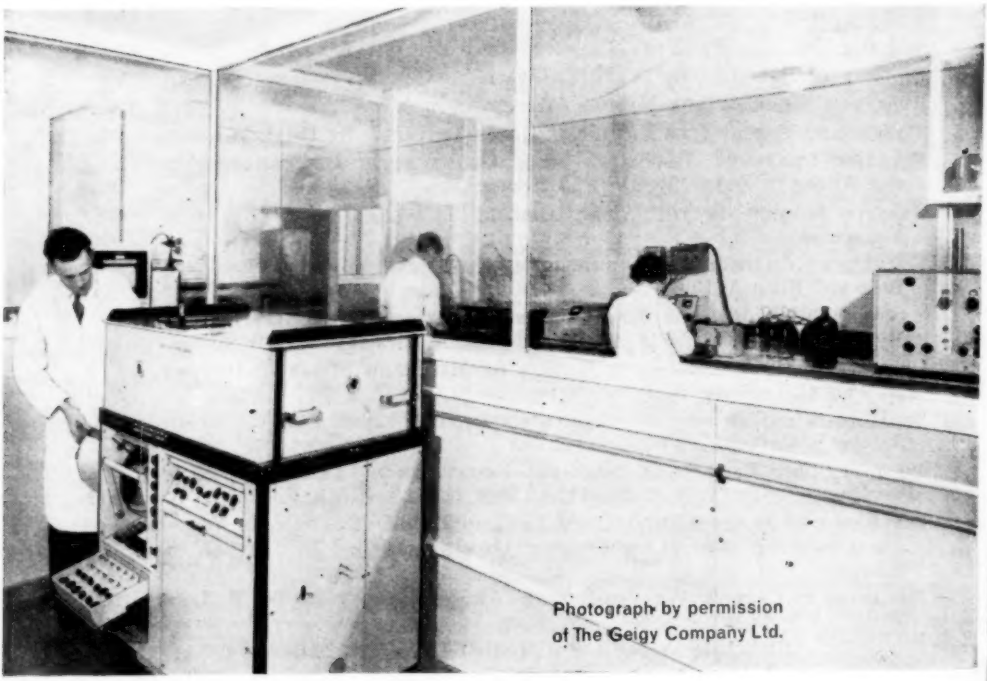

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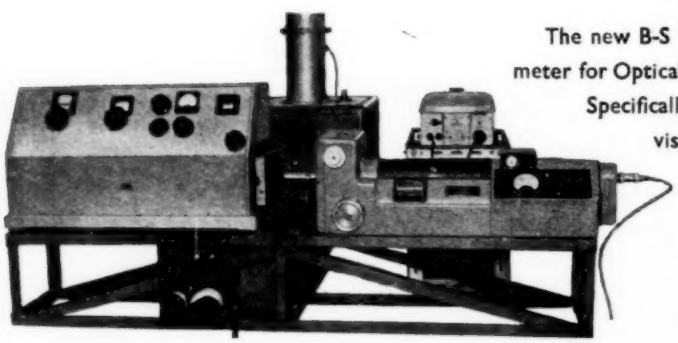

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THE URGENCY OF INTERNATIONAL CO-OPERATION

IN his presidential address to the British Association, printed elsewhere in this issue (p. 977), Sir Wilfrid Le Gros Clark touches on a recurrent theme in presidential addresses to the Association since, at its centenary meeting, Smuts referred to the way in which the scientific advance of mankind had outstripped ethical development and the need to close this gap. Perhaps even more noteworthy, however, are the similarities between Sir Wilfrid's address and that on the moral un-neutrality of science which Sir Charles Snow delivered at the meeting of the American Association for the Advancement of Science early this year. There can be no such thing as the ethical neutrality of science, for by the very nature of scientific activity, the scientist, with his over-riding respect for the truth, has a built-in moral impulse that imposed on him a constant moral discipline. This, and not the æsthetic values which the scientist shared with other creative workers is what really marked off scientific activity from other intellectual activities.

Moreover, this is not the only moral component for the international character of the scientific community was evident even as late as thirty years ago. Laying some stress on the personal links which then still existed within the international work of science, Sir Charles suggested that the exchanges which took place then were, and have remained, more valuable than all the diplomatic exchanges ever invented. While this position was shattered in nuclear physics with the development of the atom bomb, much of the international community of science remains in other fields such as biology, and Sir Charles considers that the moral and intellectual leadership of science may well pass to biologists and that among them we are likely to find the Rutherfords, Bohrs and Francks of the next generation.

It is at this point that Sir Charles links up the morality of the scientists with the problems of an organized society. Only a very bold man, when a member of an organized society, has the power to say no; but, recognizing this, Sir Charles nevertheless believes that scientists have a moral imperative to say what they know. The spring of knowledge itself provides a spring of moral action in scientific activity which is at least as strong as the search for truth; and, as Sir Charles sees it, this moral imperative could supply the driving force which would enable scientists to meet their plain duty in the present world situation and explain clearly the implications of the challenge which now confronts mankind.

Sir Wilfrid Le Gros Clark arrives at essentially the same position by a very different route. Believing that the dangerous uncertainties looming ahead make it essential to take stock of the species *Homo sapiens* whose evolution has culminated in mankind to-day, he believes also that it is our duty as scientists to insist that this is a matter of quite extraordinary urgency—and to make it apparent as widely as possible. The frightening question is now beginning to present itself, whether the civilization which mankind has slowly and laboriously built up over a period of many thousands of years can avoid disastrous dissolution as the result of uncontrollable or uncontrolled struggles for political power or economic superiority; and, indeed, whether the human species can avoid at least partial extinction by the misapplication of its own ingenuity.

Sir Wilfrid begins with a rapid review of how we came to be and how we are constituted in our physical and psychological adaptability to cope with our changing environment to-day. He notes that human evolution has by no means been a record of uninterrupted, uni-directional progress, and that probably the development of the distinctively human type of social organization depended in the first instance on the use of tools and weapons in co-operative activities for foraging and hunting for food and for the protection of family groups. This demanded an accelerated development of those parts of the brain whereby emotional and instinctive impulses can be more effectively subordinated to the good of the community as a whole. It is his thesis that the emergence of the biological species *Homo sapiens* has been the resultant of selective pressures directly dependent on the invention and development of increasingly complex cultural organizations. Stressing the importance of the period of immaturity during which the growing child remains dependent on the adult members of the community, thus making possible the full post-natal development of the large and complicated brain and also providing the opportunity for educating the individual to adapt himself to the intricate inter-relationships of human society, Sir Wilfrid remarks that if man (in the broadest sense) invented culture, it was culture which invented *Homo sapiens*.

Considering now man as he is to-day, Sir Wilfrid points out that an outstanding feature of our species is a broad and plastic adaptability rather than a narrow adaptedness to some particular set of conditions. He emphasizes that our biological adaptability needs to be constantly supplemented by deliberate and conscious efforts to adapt ourselves to local circumstances as they arise. There is some reason for satisfaction, so far as physical and biological environments are concerned, in the success achieved in utilizing the resources of the physical and biological sciences and the derived technological developments, by developing more effective methods of control of pests and protection against disease and by intensive studies of the physiological and psychological factors underlying the adaptational potentialities of the human body; but the related field of ergonomics, or the study in the widest context of man in relation to

his working environment, has as yet not been developed to anything like the same extent.

It could well be argued that here is to be found one important factor responsible for some of our economic difficulties to-day. Sir Wilfrid recognizes that the relevance of ergonomics to industry, so far at least as the rational design of machinery and the control of the immediate climatic working environment are concerned, is widely admitted; but the factor of adaptability enters no less into such matters as redundancy, arising out of technological change through developments such as automation and the application of computers and other electronic devices. It is a matter of the adaptability of both trade unions and of management, and is clearly a more formidable task than is presented on the material plane, where, as Sir Wilfrid observes, we can feel some assurance of ultimate success, provided adequate funds and facilities are made available for investigation by scientific methods.

Quoting John Stuart Mill's observation "There is not a more accurate test of the progress of civilization than the progress of the power of co-operation", he remarks of this question of mankind's adaptation to his sociological environment that, while there has been demonstrably a broad historical trend towards a progressive unification of mankind, this has come about as a result of conflict between opposing groups, ending in the emergence of the vanquished into the social and economic structure of the victor, as well as by mutual agreement for the furtherance of common interests. This process of unification has been to the ultimate benefit of the whole community by achieving a higher level of co-operation, permitting, for example, a more centralized organization of all means of production, more efficient control over local fluctuations in food supply, more comprehensive planning of large-scale projects of irrigation and transport and more economical division of labour. It can thus be regarded as a major factor determining sociological progress; but in these larger groups of human society, Sir Wilfrid fears that there is the basic danger that, with increasing pressure of population, the altruistic sentiments which in the course of a long evolutionary history have become ingrained in the mentality of the individual are too apt to be suppressed by the narrow interests of each particular group.

There have been some unhappy demonstrations in recent years of the way in which in Britain sectionalism works against the interests of the community as a whole; and it is undoubtedly true also that in the larger groups the attitudes and opinions of the individual come to be directed for him by all the subtleties of propaganda developed in a complex society, so that we have to face the seemingly paradoxical fact that the larger the units of co-operation in social communities the greater the tendency for their segregation into populations opposed to each other by conflicting economic and ideological interests. This has, as Sir Wilfrid observes, tended to slow up the historical process of unification and threatens to halt it; but before we question whether it is even feasible to hope for a final unification of the whole of mankind, it should be remembered that some of the difficulties at the international level arise precisely because not all members of the international community have reached the same level of culture or civilization. We cannot assume the existence everywhere of the altruistic sentiments to which Sir Wilfrid refers, and this is undoubtedly one factor that threatens world order to-day.

Nevertheless, Sir Wilfrid rightly maintains that an essential prerequisite for any attempt at further unification for the good of mankind as a whole is the ability to give full expression to the impulse for co-operation which is so characteristic a feature of the humanity of man, and that this can only be made possible by opportunities for the free interchange of ideas among the population of the world. At this point Sir Wilfrid expresses his conviction that science has a vitally important part to play. Regarding science and technology as culminating expressions of the co-operativeness on which depended the genesis of the form of human society in the past, he suggests that science is in a particularly favourable position to effect the closest contacts between peoples who may be divided by political and other interests. Thus he points to the recognition by scientists of all countries of the need to retain and develop harmonious relations, because the very advance of science depends on free communication. Secretiveness is a brake on such advance, because it prevents the unrestricted interchange and interpenetration of ideas among those engaged on common problems.

Sir Wilfrid thus recognizes this pattern of unity and co-operation among scientists even in the face of circumstances which often appear frustrating, and he asserts indeed that the quintessence of the scientific mentality is to be found in a willingness to communicate and discuss new advances in knowledge with completely open-minded freedom, uninhibited by any attempt at secrecy for partisan or sectional interests of any kind. Moreover, he finds that the universality in the logic of the scientific method permits an intimacy of understanding scarcely possible in fields of learning where there are fundamental conflicts of opinion derived from preconceived convictions and beliefs, based on unverifiable evidence and not amenable to scientific methods of inquiry by the rigorous test of repeated observation and experiment. How far this profound respect for objective truth, which may be regarded as intrinsically a self-regulating process, imposed on the scientist by the very nature of science, contributes to the promotion of a sense of toleration and intellectual humility may be a matter of opinion and is not discussed either by Sir Wilfrid Le Gros Clark or Sir Charles Snow; but there can be no gainsaying the points they make about co-operativeness being an essential element in the pursuit of science, and the responsibility of the scientist to explain the implications of advances in scientific knowledge. There may be reservations as to the extent to which international scientific associations can and do influence mankind generally, particularly in the face of national conflicts, but

there can be no question as to the value of associations which are wholly international in achieving full international unity. Furthermore, even where regional research institutions and other regional institutions supplement national institutions in the study of problems of common interest, membership should, as Sir Wilfrid insists, be open unconditionally to competent scientists of any community whose welfare depends on the results of the institution's research projects.

The process of dissipating tendencies towards national rivalry and engendering an atmosphere of concord may be slow—much slower than Sir Wilfrid suggests—though he rightly points out that time is getting very short. That is not, however, to belittle the importance of scientists seeking to inculcate as widely as possible a spirit of freedom in scientific inquiry which gives full expression to the deep-rooted altruism; such altruism is an essential attribute of the humanity of man, and can possibly do much to preserve and intensify the feeling of harmony among all peoples of the world. It is rather that it strikes a fresh note of urgency in the task, particularly where it involves also fostering similar altruism among some of the less-developed and emergent nations. A concrete objective is, in fact, set before the British Association, and also associations of professional scientists and technologists, namely, to make plain, in season and out, to the individual citizens and the communities to which they belong, the probable impact on everyday life of new scientific and technological discoveries. This involves demonstration of the principles and methods of scientific inquiry by which discoveries have been made, with the inferential corollary that the same principles and method can also be applied, with some assurance of success, to other outstanding problems of a similar nature. Whether or not there is still time to complete the task, it is essential that it should be undertaken wholeheartedly and urgently. Full co-operation by all scientists of all races is essential; they should address themselves to the study of those specific problems in ergonomics and other fields which bear on co-operation in industry and in society, and the way men and women live together in communities in an era of rapid and often bewildering technological change.

VERTEBRATE PHYLOGENY

Théories de l'Évolution des Vertébrés Reconsidérées à la Lumière des Récentes Découvertes sur les Vertébrés Inférieurs. Par Erik Jarvik. Préface et traduction du Prof. J. P. Lehman. (Monographies Scientifiques.) Pp. 104. (Paris : Masson et Cie, 1960.) 20 N.F.

IN the University of Stockholm at about the turn of the century, when the Department of Zoology acquired new premises they renamed it Zootomiska Institutet. This was to emphasize that the zoology studied there was not 'mere systematics', but the science of the detailed and comparative structure and development of animals; and perhaps nowhere has research been so faithful to this ideal. What has especially appealed to the Stockholm school is the development in ontogeny and phylogeny of the rhythmic plan of vertebrate segmentation and encephalization. While Holmgren, Pehrson and others made their discoveries on the living species, Stensiö and his school have been able to test the conclusions based on comparative anatomy and embryology by direct anatomical studies of fossil vertebrates.

The technique invented by Sollas—grinding and photographing, grinding again and photographing, until the specimen is destroyed, leaving a record of its structure from which wax models can be made—has been used to such effect by these dedicated workers that we know more about the detailed structure of some of the early chordates than about many a living species.

Stensiö's first work of this kind, on *Cephalaspis* (1927), was a great event. I remember his lectures on the subject at University College, London, when in deliberate, matter-of-fact tones he said of this palæozoic animal: "And so, you see, we are forced to the conclusion that this animal possessed a functional pronephros".

Jarvik, himself a distinguished contributor to this work, has written a little book summarizing such results of these many studies as have had most influence on our interpretation of the relationships of the classes and subclasses of lower vertebrates, and on the course of evolution within the series.

We have come to accept that the cleft between lampreys and hagfishes, based largely on the naso-hypophyseal relationships, represents the palæozoic cleft between Cephalaspidomorphs and Pteraspidomorphs, as Stensiö concluded in 1927. The discoveries on the Placoderms have not yet been incorporated into our text-books. Parker and Haswell in their *Text-book of Zoology* (Forster-Cooper's edition) too hastily accepted the conclusion of Watson that the Acanthodii possessed a ventral hyoidean gill-slit. Anyone acquainted with a range of Teleostean forms would know that the presence of a single row of gill-rakers on the ceratohyal was no argument for this, and Holmgren pointed this out in 1941 with additional evidence against Watson's theory. Stensiö has found both Arthrodires and Antiarchi to have a great deal in common with the Selachii, so that he unites them broadly as 'Elasmobranchiomorphi'. No one has yet used the Sollas technique on an Acanthodian, so this group is dismissed by Jarvik as *mal connu*.

Jarvik lays much emphasis on the results of his own work on Crossopterygii, which, he concludes, were already divided into two groups ancestral to Anura and Urodeles respectively. This conclusion is based mainly on the structure of the snout, analysed in detail in his work of 1941–42. It is an example of the way in which the new work has pushed divisions back further, so that the dotted lines of the phylogenetic tree, which once joined to form a main trunk, now vanish back into the unknown. On the other hand, Jarvik has allowed himself a theoretical tree in which, with the help of a few hypothetical groups, there are no dotted lines at all.

Uniformity of basic plan is re-emphasized, and one of the most interesting and elegant results is the inclusion of the dermal bones of the base of the skull in the segmental pattern of the visceral arches. *Latimeria* does not receive extended mention partly because this book is a kind of supplement to the corresponding parts of the *Traité de Zoologie*, where

this Cœlacanth receives adequate attention; but perhaps partly too because Millot and Anthony wrote their first fine volume on its anatomy without disclosing the condition and course of the profundus nerve, a great disappointment to the Swedes, since it has an important bearing on the interpretation of the homologies of the pre-mandibular somite.

The concept of de-lamination (why not supra-lamination?) and the lepidomorial theory, the use of the course of the lateral-line canals rather than the pineal to determine the homologies of the roofing bones of the skull and the abandoning of the theory of a wide osteogenic influence of the neuromasts, are acceptable generalizations which seem overdue. The origin of the paired fins by concentrations of originally broad-based segments of the fin folds receives new support from Stensiö's work on the Arthrodires in which (*mirabile dictu*), as in the Cephalaspids, the course of nerves, arteries and veins was preserved as hard encasing tubules. But the opposing theory quoted (to be demolished) by Jarvik, that the two paired fins were formed by the growth behind two of the spines of the ventro-lateral series in *Acanthodes* of a web, rays, muscles, etc., is cruder than any which I have ever come across.

The Swedish original of this timely and challenging book is one of a score of articles in *Svensk Naturventenskap 1959*, year-book of the Swedish national council for research in the natural sciences, and the translation was published without giving Dr. Jarvik an opportunity to equip it for its new purpose. Necessarily condensed and somewhat arbitrary, it drives the reader for evidence to the original papers, yet lacks a bibliography. The absence of an index is partly compensated by a good list of contents. There are few misprints (a full stop where there should be a colon in the middle of p. 31 is an awkward one). The many illustrations, including photographs and colour-diagrams, probably account for the expensiveness of this handsome paper-back, which should nevertheless be studied by every serious student of vertebrate morphology and phylogeny.

To most of us the French version is very welcome, but anyone with a reading knowledge of Swedish would have a bargain in the original, well bound, with two additional colour-plates of Jarvik's models and a number of articles on diverse subjects, all for the much lower price of 18 Swedish crowns.

E. Trewavas

SOCIAL HISTORY OF ENGINEERING

A Social History of Engineering

By W. H. G. Armytage. Pp. 378. (Faber and Faber, 1961.) 42*s*.

PROF. ARMYTAGE'S volume is the first of a series which, we are told, is designed primarily for candidates for the diploma in technology, but also for the professional scientist and engineer. The dust-jacket also tells us that the series "has been planned to help the specialist take account of the economic, social and political implications and consequences of his work and influence in society at large". Books written to this recipe are of course badly needed. The history of technology, edited by Dr. C. Singer, aroused great hopes which the published volumes have not wholly fulfilled. Their bulk and cost alone would have made them unsuitable for use by the large numbers of young men interested in the subject, or compelled to study it.

For the success of this new series the present volume augurs neither badly nor well. Books like this must serve two distinct objects—to provide a factual record of technological development in the past and to relate the development to changes in society. The two tasks require from the author a combination of aptitudes and of equipment not commonly to be found together; and it would not therefore have been surprising or unforgivable had Prof. Armytage succeeded in one of his tasks better than in the other. But this is not what Prof. Armytage has achieved. He has come within sight of both his objectives, yet the distance by which he has failed to reach either would be sufficient to damp the enthusiasm of the most indulgent of reviewers.

As a historical record the book suffers from an excess of that very quality against which Prof. Armytage warns us in his preface, that is, packaged history. Technical facts, scientific and engineering terms follow each other at the double quick. There are about 1,200 proper names in the index, and this contains only some of the names which pepper the pages. Moreover, it is precisely because he tries to tell so much that he often fails to tell us enough. Lack of space has prevented him from explaining the nature of many of the inventions and innovations and processes which he enumerates, with the result that some of the pages would be largely incomprehensible to an average student, be he even a student of technology. Now and again, Prof. Armytage is compelled to compress his facts to the point of distortion. His story of the improved harnessing of animals in the Dark Ages is told as if it concerned the replacement of oxen with horses, whereas its main achievement was greater efficiency in the use of oxen. The introduction of the open-field system in the same period is described as if it originated in the eighth century and was all organized on a three-field system from the outset. To pass to the other end of the spectrum, Prof. Armytage refers to *Zeta* as if it were a technical success, and credits Prof. Jewkes with a proposition concerning the contribution of large firms to inventions, which is exactly the opposite of the views which Prof. Jewkes is commonly believed to hold.

For the same reasons, Prof. Armytage is sometimes compelled to gloss over the technical trends which, if brought out, might have introduced order and coherence into his story of individual inventions. Thus, the technological progress in European mining is described as if it began with certain vague improvements in the 1450's. The reciprocal connexions between technical innovations in the textile industries of the eighteenth and the early nineteenth centuries receive no more than a passing reference; the linked developments in agricultural engineering are scarcely mentioned. The story of aircraft between the Wars leaves the reader unaware of the process whereby in the early thirties there emerged the first monoplanes with stressed skins, retractable undercarriages and monocoque structures. The story of radar glosses over the scientific and technical developments which proceeded and followed Watson-Watt's initiative, and there is scarcely a word in the book about Appleton, Oliphant or Roe. In the story of engineering proper nothing is said about progressive changes in working speeds and tolerances and the consequent evolution of machine tools, or about cigarette-making machinery, scientific instruments or watches and the part they played in the development of precision and automatic tools.

Much of the book's space is of course given over to non-technological matter brought in to supply the social background. Some of these data are highly relevant and valuable. Prof. Armytage's account of technical education, his story of the higher engineering schools abroad, and above all, his account of the connexions between individual engineers, who worked with whom, who established the nurseries of engineering talent, how and when the engineers began to collaborate and the professional institutions were established—all this had never been brought together so briefly and so conveniently as Prof. Armytage has done in his book. This alone would justify his effort and commend it to the general body of interested readers.

Unfortunately, the same cannot be said about many of the other non-technological facts in the book. A history of technology linking it with economic conditions, social changes and intellectual movements is something which, in the present state of the literature on the subject, no single person could be expected to conjure up; and Prof. Armytage is certainly no conjuror. The only social correlation he is able to establish at all convincingly is that of war. He has no difficulty in showing how successive wars stimulated progress in applied sciences, and above all in engineering. But many of his other correlations appear to be questionable or irrelevant. What, for example, has the destruction of drainage at Passchendaele got to do with technological changes? How does Mr. Hollerith's tea-time conversation with Dr. Billing, who had some twenty years previously collected statistics on the Army of the Potomac, establish the connexion between the American Civil War of 1861 and the invention of the Hollerith system in 1890? What has the enforced sales of British securities in America in the two World Wars ("led to increasing dependence on the American stock market") got to do with technological change in the same period? Modern architecture may or may not be part of the recent history of engineering, but if it is, its history does not begin with Gropius; and even if it owed as much to Gropius as Dr. Armstrong believes, why insist on Gropius's fleeting connexions with A. E. G. and yet fail to give an adequate account of the Bauhaus. I do not believe that a writer as pressed for space as Prof. Armytage need have bothered to bring in the Indian Mutiny, but if he did, he might have taken care not to make the Moslems venerate the cow and the Hindus abhor the pig.

These are mere examples culled at random, but they should be sufficient to explain why the reviewer's verdict is bound to remain open. The book contains some valuable information, conveniently summarized for the discriminating reader, but it cannot be recommended as a sole guide for young men about to be examined in the social history of engineering.

M. Postan

UNIVERSITY SELECTION

The Chosen Few

An Examination of some Aspects of University Selection in Britain. By W. D. Furneaux. Pp. xxvii+245. (London: Oxford University Press, 1961. Published for the Nuffield Foundation.) 25*s*.

Discussion on educational topics seems to go in cycles. After a spate of discussion, both informed and uninformed on the selection of children for secondary education and a number of published researches on the reliability, validity and sociological consequences of such selection methods, it is now the turn of the universities' selection procedures to be closely scrutinized. These provide a more formidable task for the investigator; the fewer university bodies with their particular faculty requirements are often less co-operative than the 146 local education authorities with their eleven-plus selection procedures. As the author of the present book points out, this lack of co-operation is often not deliberately so, but is due to the lack of availability of records and information about students who are accepted or rejected for a university course.

This research began in 1948 under the auspices of the Nuffield Foundation and was carried out by the Nuffield Student Selection Unit of the University of London Institute of Psychiatry. Access to relevant records was not granted by Oxford and Cambridge, and the main research was centred on the University of Sheffield. There is no evidence to show that this restriction provided a satisfactory sample of university entrants. One point does emerge clearly; unless each university endeavours to build into its selection scheme some follow-up measure of the trustworthiness and validity of its selection methods, the continued wastage of time and talent will continue. Interviewing candidates as a selection instrument is still as unreliable as it was found to be by Hartog and Rhodes in their report in 1935. With increased number of university applicants and the correspondingly greatly increased wastage of time by university staff in considering applications, alternative psychological methods will have to be considered and tried out. As Furneaux says of the academic staff responsible for selection: "In general they set about their work conscientiously and with devotion, but are guided by a mixture of empiricism, tradition, and personal experience, which cannot always be expected to produce the most valid results".

According to the author, 'A' level results constitute "the only reasonable and available objective evidence", but these results do not reach the selection panels early enough and even then, because of the difficulties of assessing the value of 'essay-type' questions, are not very efficient predictors. It is pointed out that the main function of 'A' level results is to confirm "that the minimum statutory entrance requirements have been complied with"; however, the recent report by J. A. Petch does show that the General Certificate of Education examination has some predictive value. One rather gathers that preliminary acceptance of a student by a university owes a great deal to the skill shown by the headmaster in the writing of his report, nevertheless, Furneaux believes that more use might be made of these headmasters' reports, if the universities would use a more precise method such as a questionnaire for eliciting relevant information.

This report underlines all the known difficulties of student selection and criticizes, at least by implication, the waste of time and effort on students' multiple university applications; it points out that there seems to be no clear evidence that the standards achieved by students admitted to a university and those applicants who were rejected by this university and admitted to another, differ in any marked way.

Points raised in Part 1 of this study relate to multiple applications, sociological background of students as a determinant of selection and wastage of talent. Part 2 deals more specifically with the Nuffield investigation. This is a useful source book, though finding one's way through the various facts

XUM

and arguments, often repeated and expanded in various sections of the book, is not too easy even with the help of the index; this may be due to the ten-year time-lag between the initiation of this study and its final publication. However, few will disagree with the main points of the author's thesis, and his plea for some form of clearing-house system is timely.

Psychologists and sociologists will look forward to the second volume, and now that Mr. Furneaux has dealt with the background of selection he will be free to deal with the ways in which psychological tests and procedures can be of use in university selection.

W. H. King

THE PHYSIOLOGY, PHARMACOLOGY AND PATHOLOGY OF MUSCLE

The Structure and Function of Muscle

Edited by G. H. Bourne. Vol. 2: Biochemistry and Physiology. Pp. xiv + 593. 118*s*. Vol. 3: Pharmacology and Disease. Pp. xiv + 489. 107*s*. 6*d*. (New York: Academic Press, Inc.; London: Academic Press, Inc. (London), Ltd., 1960.)

THIS treatise on muscle is completed by the present volumes. In Volume 2, the first two chapters, on the chemistry of myofibrillar protein and on the biochemistry of muscular action, set a high standard. Chapter 3 gives an account of the biochemistry of muscle mitochondria, and is followed, in Chapter 4, by an essay on endocrinology with reference to muscle. Chapter 5, entitled, "The Neuromuscular Junction: Role of the Acetylcholine System", is a discursive review of some aspects of conduction, neuromuscular and synaptic transmission. Chapters 6 and 7 give a concise account of the biophysics of muscle, and are followed, in Chapter 8, by an excellent survey of the comparative physiology of the activation of muscle. The next two chapters are essays on human muscular physiology, with emphasis on factors limiting performance and on fatigue. Chapter 11 is a full and balanced review of the central nervous control of skeletal muscle, which covers the substantial advances made in the past two decades. This volume is concluded by a stimulating chapter on certain aspects of the physiology of cardiac muscle.

It is regrettable that much space has been given, in Volume 2, to marginal topics, such as endocrinology and the biochemistry of sarcosomes, apparently at the expense of an adequate treatment of the physiology of smooth and cardiac muscle. A disappointing feature of the book is the section on neuromuscular transmission, which unduly emphasizes the personal views of the author. Fortunately, these faults are offset by the uniformly high standard of most contributions to this volume, which can be recommended to the advanced student and teacher as a useful account of many of the recent advances in the biochemistry and physiology of muscle.

In Volume 3 two chapters are devoted to pharmacology: Chapter 1 is a review of the actions of drugs on smooth and skeletal muscle; in Chapter 2 some aspects of the effects of drugs on the myocardium are discussed. Virus infections involving muscle are described in Chapter 3, which is followed by a chapter on the parasitic infections of muscle, with emphasis on the widespread prevalence of sarcocystic infestation. An illustrated account of the degeneration and regeneration of skeletal muscle in Chapter 5 is succeeded by chapters on the effects of nutritional deficiencies on muscle, and on the histological changes in muscle due to ageing. Chapter 8 is an authoritative review of recent advances in the study of post-mortem changes in muscle, with especial reference to rigor mortis. The histochemistry of muscle is dealt with in Chapter 9. The pathology of the intrinsic diseases of muscle is described in Chapter 10, which is followed by a chapter on the clinical features of these diseases. Chapter 12 is an interesting discussion of the genetic aspects of muscular disease. A brief but stimulating essay on some fundamental problems of muscle biochemistry concludes the work.

The article on the pharmacology of skeletal and smooth muscle is outstanding, in Volume 3, in its conciseness, and will be of great value to the general reader. It seems remarkable that, in the treatment of pathology, parasitic and virus infections are considered in some detail, but no mention is made of Clostridial infections. The quality of the histological illustrations is not uniformly satisfactory: many are too small and suffer from poor definition. Nevertheless, this volume may particularly interest clinicians, and should focus attention on a field which has hitherto been rather neglected. D. H. Sproull

ECTO- AND ENDO-PARASITES OF SOUTH-EAST ASIA

Malaysian Parasites

XXXV–XLIX. Edited by W. W. Macdonald. (Studies from the Institute for Medical Research, Federation of Malaya, No. 29.) Pp. vii + 251. (Kuala Lumpur: Institute for Medical Research, 1960.) 9 Malayan dollars; 21*s*.; 3 U.S. dollars.

THIS is the third volume of a series of collected papers on the ecto- and endo-parasites (including biting insects and arachnids) occurring in south-east Asia. It contains two long papers, both by C. B. Philip and W. W. Macdonald, on Tabanidae and Culicinae respectively, a review of the host distribution of Malayan ticks by J. R. Audy and others, and several shorter papers on mites, together with one on the rearing of ixodid ticks and another on nematode parasites of domestic rats.

Like its predecessors it is well bound, clearly and attractively printed and illustrated, and remarkable value for money. Most of the papers are for the expert, but Macdonald's paper on the ecology of mosquitoes in lowland dipterocarp forest should interest the general reader. The same author's paper on *Armigeres* sub-genus *Leicesteria* also contains much of ecological interest, with particular reference to the fauna of bamboo internodes.

It is sad to think that most of those who worked in the Institute during the publication of this series have now dispersed. Their achievements bear witness to the unique potentialities of a team of this kind. The present staff can be trusted to carry on the tradition within the limits of their resources. For the rest it is to be hoped that visiting experts will use the Institute as a centre for exploring the marvellously rich fauna of south-east Asia and the many intricate and still little understood problems of human ecology which the area presents.

P. F. Mattingly

ART AND THE ENGINEER*

By Prof. MISHA BLACK, O.B.E.

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NEGLECTING the massive contrary evidence, many engineers continue to console themselves for a lack of creative ability by assuming, and constantly reiterating, that form inexorably follows function and thus that a solely scientific or technological approach to engineering produces forms which are visually so satisfactory as to eliminate the need for conscious æsthetic endeavour. The protagonists for this view point with pride to high-speed passenger aircraft as proof of their theory, and extend the range of their evidence by mustering the hulls of ships, the control rooms of power stations, electronic equipment and the mechanism of intricate machinery to their support.

One must reluctantly counter these admirable bids with the lamp standards which still destroy the visual amenities of a thousand streets, most rail and road bridges built since 1900 and the myriad consumer and capital products which have gone far towards destroying the pleasures of city life and have eroded the rural scene.

This balancing of the good against the unsuccessful would be an amusing game if the stakes were less than our total environment, which they in fact represent.

Thus I am producing these over-simplified arguments which before now should have been transferred to a more serious level of discussion. Those who might be expected to represent the opposing views have long past composed their differences. Sir Herbert Read (whose loyalty to the arts is unquestioned) wrote in 1934[1]: "One false theory assumes that if the object in question performs its function in the most efficient way possible, it will *ipso facto* possess the necessary æsthetic qualities. To this argument we must reply that an object which functions perfectly may, and probably will, possess æsthetic qualities, *but the connexion is not a necessary one*".

The late Oscar Faber (whose engineering experience is beyond dispute) said in 1944 (in a paper delivered at the Institution of Civil Engineers)[2]: "The fallacy, which is very widely held, is that if a structure is honestly designed to satisfy all its scientific or engineering requirements it will of necessity be beautiful . . . all I can say is that I wish it were true, because most engineers can design a structure scientifically with a constant factor of safety in all its component parts, and if that were a guarantee that the result would be beautiful the problem would indeed be a simple one. Unfortunately, all that one can say about it is that such a structure may by chance be beautiful, but it will only be by chance". Yet at every conference on civic or industrial design the ancient ritual is repeated, and at least one engineer can be relied on to chant the now venerable refrain that form automatically follows function, that mechanical efficiency produces its own art form and that association with the arts is advisable only outside the drawing office and workshop.

This dogmatism on the part of some engineers has sustained the equally emotional opposition of humanitarians. They, from William Morris onwards, imagining their values threatened by developing technology, have retreated (as the forces of science are too virile and powerful for open battle) historically into the peaceful backwaters of arts and crafts and to-day into art forms which express their own despair and have meaning only for those who understand their obsessive pessimism.

The continuing vigour of the argument results from there being right on both sides. There is no doubt that the engineers of the twentieth century have produced the most memorable symbols of our time which, when examined in the perspective of future centuries, may well prove to be more important manifestations of our then past than all but a few of the more self-conscious works of our architects, sculptors and painters. But it is equally true that the majority of the works of our civil and mechanical engineers are characterized by a mediocrity only equalled by the mass of our architecture. The engrossing mystery is the unevenness of quality which can be found in the work of even a single homogeneous group of engineers, the circumstances which make it possible for the designers of a superb flying machine to produce cabin interiors redolent of a shoddy suburban hotel and for the designers of an efficient and economic street lighting system to mount the fitting on poles whose only excuse is that others are yet more disagreeable.

At the Frontiers of Knowledge

The answer to this inconsistency lies in the extent to which the engineer is working at the frontiers of knowledge. When the problem set is more technically exacting than any attempted before in that field, then the discipline of the problem, the need to probe beyond existing boundaries of knowledge, produces formal results which exactly mirror the period of history which made that technical development possible. They thus have a quality of exact correctness which is classically satisfactory. Because it was produced at the advanced headland of a civilization, it could only be materialized by men who were, for that moment, at the head of their time and through them their society expressed one aspect of its greatness. Such work crystallizes the essence of a period and, on a subsidiary plane, is comparable with the fine arts which similarly express a society annealed through the fire of a single personality.

Because such work is done at the frontiers of experience its result may not always be generally comprehensible, especially in periods of revolutionary

* Substance of a lecture delivered at Hatfield Technical College on June 23.

transformation, but during the post-industrial revolution period, which is my concern here, the gap of time between achievement and public acceptance has progressively decreased. Thus to-day the creative jump forward which produced the *Caravelle* jet aircraft required no period of gestation before it was accepted as a proud symbol of the first year of this decade.

An appreciation of this view is obscured by the problem of early steps ahead into undeveloped technological ground when the creative probes produce unresolved intermediate solutions which appear primitive and immature in comparison with the then fully developed earlier forms which they are destined later to supersede. For example, one may remember the primitive forms of the early steamships as poor cousins to the then still extant majestic sailing vessels, the tottering early aircraft and the first motor-cars a parody of their future.

But though at times the early forms of a new technology may seem beautiful only to the enthusiast, the basic fact I think remains. Form follows function when the form is that of objects or mechanisms which are at the height of their time when the need to resolve scientific and technological problems exclude all other considerations and when the consciousness of the engineers is completely absorbed within the superhuman task of reaching ahead into the then unknown.

The purity of design which is possible in such circumstances is, however, a rare occurrence. It is hindered by the necessity for its expression at a stage of technological development which allows for mature solutions, it requires, in mechanical engineering, the opportunity for development of a completely new product instead of the more usual modification to part of an existing object or mechanism, and it postulates a problem being attempted in which the needs of human usage are of secondary importance. To this last point I shall return, but first may I complete this part of my argument by re-stating that engineer-designed objects of the type I have been describing possess an intense emotional content. If all the artefacts of our society were produced in this way, the problems we are considering and the need for this article would not exist. But they are not. The vast majority of engineering projects and products are conceived well within the boundaries of scientific knowledge and technological experience; the developments in a new project are commonly marginal—a minor step forward. In such engineering undertakings the possible variations multiply and usually reach infinity. Here conscious judgment, choice and constant decision face the engineer, and in these decisions æsthetic choice plays, as I hope to demonstrate, a major part.

The Clue to Confusion

Here is the clue to present confusion. The engineer rightly sees that the major 'break-through' works possess great beauty, and he is sometimes arrogant enough to believe that the qualities of great achievement must automatically be reflected in his more pedestrian daily tasks. He does not, in this context, accept the common knowledge that quantity affects quality, and that what operates satisfactorily at one scale does not necessarily do so when the scale is greatly increased or diminished. The beam which properly bridges a 20-ft. span should not simply increase in size when the span increases to 200 ft.—and the reverse is equally true. The engineer developing a motor-car which is only a stage in advance of last year's model is not operating at the frontiers of knowledge; the electronic engineer reducing the depth of a television tube is similarly working at a lesser pitch; the designer of a gas cooker or a Diesel locomotive is also operating at a level where application is more appropriate than vision. At this, the normal daily working level, the differences in design of objects produced to the same general performance specification demonstrate the freedom of choice which exists, within, of course, controlling technical conditions.

This is, I believe, indisputable. It is a truism which only the strange lack of its appreciation justifies repetition. I do not under-estimate the importance of the controlling factors of materials, manufacturing processes and cost; but these provide only the matrix within which other motives also determine the engineering decision. In domestic appliances the plethora of refrigerators and washing machines, for example, indicates how wide is the choice which faces the engineer even when the technical requirements have been established with reasonable exactness. The varieties of kettles, automobiles, vacuum cleaners and Diesel locomotives reveal major differences in ease of operation and of appearance which cannot be explained only by variations in specification or production facilities.

The engineer is a decision-making animal, and it would therefore be useful to examine the motivators of his decisions.

Motivators for Decisions

I believe that the majority of engineering decisions, within the boundaries of technical facility, are made from emotional and seemingly inconsequential premises. The radio engineer arranges the controls of a television set in relation to the screen mainly because he thinks 'it looks better that way': the draughtsman selects a particular form for a domestic radiator because he prefers that form to any of the thousand alternatives that exist within the specification of his task. The final appearance of a modern machine tool is as much the outcome of these proliferating individual æsthetic decisions as was the form of a nineteenth-century beam-engine elaborated with the Grecian columns and the cast-iron filigree which the engineer of that time similarly considered the appropriate (if then less inhibited) æsthetic decision.

This concern of the engineer with æsthetics, with the appearance of things, is now firmly established. It manifests itself in a desire for neatness, for invisible fastenings, for the compact and orderly disposition of units. It works towards the attainment of an elegant engineering solution in which the most simple economic solution is commonly jettisoned for one which has required difficult development to effect the elegant solution as efficiently and as economically as the more immediately obvious brash commonplace one.

During the past fifty years, however, a disturbing element has been introduced into this technological development towards what Whitehead[3] has called ". . . the most austere of all mental qualities . . . the sense of style". The serious searching for the most fitting solution is now commonly influenced by a real or supposed knowledge of market requirements, and engineers have been known to reject both the com-

monplace and the elegant solution for one which is neither in order to satisfy their sales staff, who are convinced that only falsification and bogus elaboration will be acceptable to their markets.

If it is accepted that the engineer commonly makes æsthetic decisions, it is reasonable to ask whether he is fitted to do so. The answer must certainly be that he is only sometimes so fitted. I have already excluded the pioneer design projects from this consideration as almost unconsciously determining their own form, but for the rest the ugliness which excretes over our streets and homes, offices and workshops is a disquieting reminder that all is not yet well. For this the engineer is not to be blamed. He has been educated, as have people as a whole, with almost complete disregard for the cultivation of any visual sense—he has had little experience in producing or appreciating the visual arts. He has been encouraged to develop his eyes into acquisitive organs concerned only with selecting for conscious recognition those aspects of the world which are of material benefit to him. He has learned to see a lamp-post to avoid bumping into it, but not to see the lamp-post so that he knows and takes pleasure or offence from its shape.

Perceptive vision has been succinctly described by Suzanne Langer[4]: "What we see in this way becomes simply a thing of vision—a form, an image. It detaches itself from its actual setting and acquires a different context". The engineer has been given little, if any, training in the technique of looking and seeing, of the minute examination of objects, or in the sense of touch and the co-relation between visual and tactile sensations.

Slowly that situation is changing as the importance of art in general education is gaining recognition, but the average graduate engineer is still visually untrained, still partially blind and then finds himself, in that twilight of cataract, faced with the need constantly to make æsthetic, formal, visual, three-dimensional decisions. In this situation the surprise is not that so much is second rate, but that so much achieves satisfactory visual standards.

A Cure for Blindness

A cure to this problem could theoretically be found in an adjustment to the curricula of engineering students which corrected the faults of their primary education and imbued their time at university or college of advanced technology with the humanities. These human studies, it has been argued, could be biased towards the visual arts so that the graduate engineer became able to make more expertly those æsthetic decisions which must later constantly confront him.

This could satisfactorily deal with the problem if it were not for two at present insurmountable difficulties. First, the knowledge required for a primary engineering degree can only be mastered by application to essential studies which leave no time for a still further extended curriculum. There is already insufficient time after basic theory has been taught to allow for the development even of primary engineering design and for application to the technique of making decisions based even on purely factual information and specification. Secondly, the present too early division of boys and girls into scientific and humanistic specialization tends to dissuade from the scientific stream those very students who might be able to assimilate a degree course in which the arts are co-ordinated with the sciences.

The immediate answer must be found, I believe, in the training of a special type of engineer who, from the commencement of his technological education, is encouraged to concentrate on that aspect of engineering which is now commonly absent from the curriculum. He must be what in our civilization is considered to be an odd character, part engineer, part artist. He must have a feeling for form analogous to that of the sculptor or architect, he must be aware of the emotive effect of colour, he must be sensitive to the tactile qualities of surface, and, at the same time, he must be able to draw and calculate, to talk to mechanical and production engineers in their own terms and on their own ground. He must be sensitive to the society he serves, conscious of the movements in public taste, excited by problems of ergonomics and anthropometrics.

If he were all these things in full measure he would be a superman, the impossible Leonardo da Vinci of the twentieth century. But I am not, of course, suggesting a course of training for supra-engineers. My aim is more modest. Those undergraduates whose training is emphasized towards what the Americans call 'human engineering' are only partial Leonardos. Alone they are useless and even dangerous in their superficiality, but teamed with those whose training and experience have been differently directed towards technological specialization, they can provide the link between technology and humanity, between those who design the machines and those who use them.

A start has been made. A few art schools and technical institutes in Great Britain are now experimenting with training of this kind. This training is still in the elementary stages in which the instructors are groping for the teaching techniques which will achieve their known objectives, which will produce graduate engineers who can complete the production team.

The Industrial Designer

A title must be found for this comparatively new specialization, which has so far reached a degree of maturity only in the United States. There they are called 'industrial designers', and that name, with all its obvious limitations, must serve as an internationally accepted nomenclature. It is more fitting, by its very ambiguity, than the German '*Formgeber*' or the French '*estheticien industriel*'.

If we accept the need for such specialized technicians, the 'missing technician' as John Gloag called him in 1945[5], it would be useful to consider how he can best function in the industrial complex. To this, as to every question in which technology directly affects human needs, there is no simple positive answer. The answer shifts its position as circumstances change.

In the design of capital goods, machine tools, special duty machinery, locomotives and industrial electrical equipment, the industrial designer's function should be a minor one. He should be a watchdog for the operator, a worrier about detail, an expert in anthropometrics and ergonomics; he should be concerned with problems of calibration, communication and psychological well-being. He should be concerned also with finish and maintenance, with the form of the total machine so that its appearance reflects the excellence of its mechanical operation. In

such cases the industrial designer is no more than the guide dog of a gifted blind man. His function is essential, but it is subservient to the needs of his master, the engineer.

In consumer products the emphasis shifts. In the design of a typewriter the human needs of the typist are as important as the mechanical efficiency of the machine. The assembly of the keyboard, the shape of the shift key, and ease of operation, become vital factors in the total design. The appearance of the machine becomes a vital element in its acceptance, and the work of the industrial designer begins to equal in importance that of the designer of the typewriter as a printing machine.

In domestic appliances the emphasis shifts still further into the territory of the industrial designer; into the strange world of consumer acceptance and product death-dates, where what the housewife will or will not like in her kitchen, what the marketing division believe they can and cannot sell, reach heights of monumental irrationality. This is a hinterland of personal preferences and peculiar dislikes, of motivation which often has but little to do with the real value or efficiency of an appliance. It is a publicity-aggravated dreamland in which the industrial designer is able to function, sustained by training and experience which have steeled him to its vagaries, and from which he can produce a reality which can materialize the highest standards society will accept instead of, in despair, looking for the lowest common denominators.

Style, Design, Obsolescence

The reservations of an engineer when asked to accept an industrial designer as a colleague are understandable. They arise first from the human weakness which suspects that this new appointment is a criticism of his own ability. This is a comparatively simple problem of human relationships which time will help to resolve. An aeronautical engineer does not question the advisability of working with a stress engineer, or with experts in hydraulics, electrics and other branches of intense specialization. Few architects doubt the wisdom of collaboration with civil engineers; few surgeons would be prepared to operate without an anæsthetist.

The other cause for lack of enthusiasm for collaboration is more serious. It arises from the engineer's deep distrust of the whole operation of industrial design. He suspects that the designer is concerned purely with superficial style, with fashionable frippery which is completely separated from, and sometimes detrimental to, the function of a mechanism.

At its most extravagant this design concept is expressed in some automobile design in which the body-work is disagreeable and in which spear-pointed exuberances turn an innocent vehicle into a lethal pedestrian-hating weapon. It is expressed in kitchen equipment which is so loaded with gadgets as to require the housewife to turn herself into a skilled mechanic (or to disregard the elaborations which she has expensively bought), and which is a pleasure only to the service engineer who is kept busy repairing this unnecessary complexity.

But having properly disparaged such baroque styling, the need for style remains.

To reduce the problem to its essential crudity: 'things are made for people'. Human needs, desires and ambitions must be the arbiters of all industrial production. There is no gain in manufacturing an excellent piece of equipment which no one needs or wants and is therefore not prepared to buy. This is a truism for any society as soon as the earnings of any large section of the community exceed their minimum requirements for shelter and food. It is already manifest in the U.S.S.R., where the increasing supply of 'non-essential' consumer products such as television sets is creating the problems of consumer demand or resistance which have for long been a characteristic of Western marketing. This sensitivity to human needs and the relation of the form and operation of engineering products to the style which mirrors each facet of a society at each moment of its complex and uneven growth is an attribute of the skilled industrial designer; it is one of his specialized functions.

The reasons for the disrepute of this quite proper function, about which the industrial designer is as sensitive as the engineer is scornful, is that too often the body-work of industrial products has been 'styled' by self-styled designers whose talents reside only in their ability to produce competent perspective sketches which satisfy the uncritical judgment of industrial management and, often, disregard all production and servicing desiderata. This process has become self-propagating as the engineering staff have always managed to find a method of producing the stylist's extravagances; a sufficiently large market has been satisfied by these monstrous objects which has become status symbols and, in the competitive battle for markets, the gadgetry of one manufacturer has been capped by another's still wilder bid for applause and profit.

The situation remains with us; but the tide shows signs of turning. The industrial designers are becoming serious and expert. They have largely renounced the extravagant showmanship which was their defence during the period when their battle for recognition was more bitter than it now remains. They are being trained to collaborate with their colleagues in the factory production team and are no longer expected by industry to produce magical rabbits at the drop of a commission. They are as concerned with production problems as are the production engineers themselves; they have willingly discarded their artist's freedom for the discipline of the factory, hopeful that serving a social purpose will compensate for the compromise this almost inevitably imposes.

Scientific Romanticism

I want, lastly, to consider an aspect of this problem which inhibits an appreciation of it in what I believe to be a proper perspective. Those designers who have accepted their essential liaison with science and industry have sometimes transformed what should be understanding and sympathetic friendship into too passionate a love affair. To quote from a recent issue of the Italian publication *Zodiac*[6]: "The important art of the twentieth century is going to be produced by the men who understand our age and who create artistic objects which are useful to-day. Those men who create these objects are the true artists of our time". In his fundamental book, *Art and Industry*, Sir Herbert Read wrote: "Such beauty as we admire in Greek vases, and in the undecorated forms of Mediterranean art generally, can undoubtedly be produced by machinery".

I wish that I could accept these adulations of the industrial designer. It would be pleasant to believe that a stainless steel coffee pot made by the hundred

thousand in a Midlands factory could have the same quality as one of Isaac Dighton's silver pots of the early eighteenth century. But the mass-produced pot, however appropriate and satisfying its proportions may be, however well it may be made, however efficiently it may operate, however good the quality of its metal and finish, lacks the one quality essential to a work of art. It lacks uniqueness. That is a quality which every natural organic object possesses: it is the quality which every hand-made artefact possesses, it is the quality which in great works of art gives them a distinction which justifies man as a creator, as a god on earth.

That quality exists in the original model which the engineer and the industrial designer may together produce, but it must be ironed out and tooled away so that the machine may produce its thousand copies without the stigmata of individuality which would make a mockery of the very process of industrial production.

In almost completely obliterating uniqueness the machine obliterates humanity in its production. The formal mathematical qualities of form and proportion remain, the sense of pleasure in using an efficient object remains, positive satisfaction in handling a well-made thing exists, but these are not, in themselves alone, the qualities of a work of art.

For compensation, we have the multiplication of machine production. If the æsthetic quality of a motor-car cannot equal that of even a minor sculpture by Henry Moore, the faint æsthetic pulse in the dehumanized forms of the automobile is multiplied by the number of cars made from the same mirror-polished tools. In quantity the mild æsthetic pleasure each car evokes adds up to an important factor in our daily lives. Multiplied by the million objects of our environment, their effect on our lives begins to equate with the impact of a single work of art.

All the works of all the designers for mass production cannot equal one Rembrandt self-portrait or a Cézanne still-life, but that need not deny us a modest satisfaction in producing, if we eventually do, an environment in which, against our machine-created background of neutral and agreeable objects, the painters and the sculptors and the architects and the great engineers can produce their masterpieces.

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COAL IN OXFORDSHIRE

By Dr. C. J. STUBBLEFIELD, F.R.S., and Dr. F. M. TROTTER
Geological Survey of Great Britain

THE Geological Survey of Great Britain[1] has implemented a programme of boring during each year since 1948. The programme is undertaken independently of the investigations carried out by national boards, industrial concerns and private bodies; and the purpose of the boreholes is to obtain a fuller knowledge of the geology of Great Britain. Should raw materials of commercial value come to light from this work, the information concerning them is made available to the public so that other organizations, if they so desire, may explore further and exploit them.

In order to preserve a balance between short- and long-term research, in some years it has been the Geological Survey's policy to drill shallow boreholes so that that knowledge of the sub-surface geology may be increased in areas covered by 1-in. maps which at the time are being officially surveyed geologically. In other years boreholes are planned for use in long-term investigation of the underground geological composition and structure in extensive areas of Britain covered by Mesozoic rocks and to check the results of geophysical researches; in general these are deeper boreholes.

Among the several regional studies now in hand, one concerns the nature of the sub-Mesozoic rocks in the area between the Severn basin and Oxford. In this area two exploratory boreholes—both were less than 2,000 ft. in depth—had been made in earlier years by industrial concerns; the first of these boreholes was started in 1872 and finished in 1875 at Signet, Burford (National Grid Reference 42/251101); strata classified as Coal Measures were entered at 1,174 ft. and boring continued in them to a depth of 1,409 ft. (ref. 2). The borehole was stated to have encountered a coal seam at 1,350 ft., but its thickness, presumably less than a foot, was not recorded and no samples of strata were kept. The second borehole, 15 miles farther north at Batsford, Lower Lemington (Nat. Grid Ref. 42/215346), was started in 1901 and completed in 1904; beneath Triassic rocks at 1,021 ft., it passed through Coal Measures for 525 ft. and then traversed Silurian mudstones to 1,700 ft., at which depth boring stopped[3].

The first Geological Survey borehole in this area was started in 1949 at Stowell Park (Nat. Grid Ref. 42/084118) about ten miles west-north-west of Signet, Burford[4,5]. This borehole confirmed the existence of a deep basin of Mesozoic rocks underlying the Cotswolds and the Vale of Gloucester, and drilling stopped in 1951 at a depth of 4,503 ft. without the base of the Mesozoic (Triassic) rocks sequence being reached.

The Geological Survey then turned its attention nearer to, and slightly north-west of, the site of the 1872–75 Signet borehole. Boring commenced in 1952 at Upton, Burford (Nat. Grid Ref. 42/232131). The Upper Coal Measures were reached beneath the Triassic rocks at a depth of 1,107 ft. (733 ft. below Ordnance Datum) and were proved to a thickness of 2,660 ft.; a few thin coal seams were found, but owing to technical difficulties the bore stopped at a depth of 3,767 ft. in July 1953[6,7].

For some years the area between the Severn and Oxford had been the subject of gravity, magnetic, and limited seismic surveys by the British Petroleum

Co., Ltd.[8], and although the Geological Survey[9,10] announced its intention in 1955 that boring might be continued at a site or sites progressively eastwards from the Cotswolds, its borehole activity there was deferred for some years pending the results becoming known of future boreholes by the oil company in that area. The first of these, a borehole drilled in 1955 at Faringdon, Berkshire (Nat. Grid Ref. 41/323940)[11,12], revealed Lower or Middle Old Red Sandstone (Devonian) rocks beneath Triassic at a depth of 2,150 ft. (1,852 ft. below O.D.) and another borehole drilled in 1958 for the Gas Council at Noke Hill (Nat. Grid Ref. 42/539129)[12] five miles north-north-west of Oxford found Upper Old Red Sandstone underlying Mesozoic rocks at about 380 ft. (66 ft. below O.D.).

When these results became known and no further boring by the British Petroleum Co. seemed likely in the early future, the Geological Survey started a borehole in August 1960 at Apley Barn, north-north-east of Witney Park (Nat. Grid Ref. 42/344107)[13].

The surface-level is 280·1 ft. above Ordnance Datum and the borehole commenced in the Great Oolite White Limestone. Various subdivisions of the Jurassic were proved, including the Marlstone Rock-bed (the top of the Middle Lias), which was 1 ft. 9 in. thick with the base of the bed at 171 ft. 9 in. In passing it may be noted that the Marlstone Rock-bed worked for ironstone north-west of Banbury at a thickness of about 20 ft. has thinned at Witney, as it does farther north towards a north–south line through the Vale of Moreton and the Vale of Bourton. The base of the Jurassic was found at 564 ft. and that of the Rhætic at 592 ft. 8 in.

The base of the Keuper Marl was penetrated at 760 ft. 6 in. and the sandstones below have been assigned to the Keuper Sandstone. Reddened Upper Coal Measures underlie the Keuper Sandstone without visible unconformity, thereby rendering difficult the fixing of the demarcation line; provisionally the base of the Keuper Sandstone is taken at 820 ft. 10 in. Reddening of the Upper Coal Measures persists to a depth of 927 ft. The first seatearth type of mudstone was encountered at 907 ft. and the first coal seam, 4 in. in thickness, at 1,005 ft. 9 in. Between 1,000 ft. and 1,948 ft. twenty coal seams were proved, and in this account these measures, which are principally mudstones, may conveniently be termed the Uppermost Coal Group; the total thickness of coal found in this Group was 21 ft., including one 3-ft. seam of coal with its base at 1,661 ft. 6 in. From 1,948 ft. to 2,602 ft. dominantly argillaceous measures included mudstones with ironstones, seatearths and four thin coal seams; in these strata, red, brown, purple, yellow, green and variegated measures alternated at about 20–50 ft. intervals with strata that were essentially grey, and these measures are called for convenience the Coloured Measures Group. These coloured relatively barren measures were underlain by 270 ft. of grey mudstones with subordinate sandstone bands to 2,872 ft. and they contained eleven coal seams and associated seatearths; these measures are here referred to as the Second Coal Group. The total thickness of coal in the Second Coal Group is 20 ft. 9½ in., and worthy of mention are a 2 ft. 7 in. seam at 2,644 ft., a 3 ft. 2 in. coal (including a 1-in. parting) at 2,689 ft. 8½ in., a 3-ft. coal at 2,749 ft. 3 in. and a 2 ft. 9 in. coal at 2,818 ft. 9 in.

The Second Coal Group is underlain by arenaceous strata of Pennant sandstone aspect which nevertheless contains eight coal seams; these measures are here called the Arenaceous Coal-bearing Group and the total thickness of coal in this Group is 14 ft. 2½ in. Particularly noteworthy are a 6 ft. 1 in. seam at 3,633 ft., of which the bottom 4 ft. 11 in. are coal, and a 6 ft. 4½ in. seam at 3,786 ft., of which the bottom 4 ft. 9 in. are coal. The base of the Arenaceous Coal-bearing Group is taken at 3,968 ft. 7 in. Fossil plants and non-marine lamellibranchs have been collected at intervals between 841 ft. and 3,773 ft., and a fish tooth at 3,943 ft., all indicating Upper Coal Measures age. A pulmonate gastropod was found at 877 ft. and in British Coal Measures such forms have been previously found only in the Keele Beds, which are the uppermost Coal Measures in North Staffordshire, and in the Coal Measures of Upton, Burford. The sequence between 2,872 ft. and 3,968 ft. contains sandstones of a type known in the Pennant Measures of Bristol and Somerset, and South Wales.

No evidence of older Carboniferous rocks was found in the borehole, for underlying the Arenaceous Coal Group is a Conglomerate Group comprising multicoloured sandstones, breccias and conglomerates with subordinate purplish and green mudstones; between 3,971 and 4,596 ft. Devonian fossils were found, including a coral, crinoid stem columnals, brachiopods and fish scales; between 4,511 and 4,562 ft. the flora indicates Middle or Lower Devonian age. Boring ceased at 4,942 ft.

The stratal gap between the Keuper and the Upper Coal Measures and that between the latter formation and the Devonian are not associated with discordance of dip. The highest dip recorded in the Devonian is 4°.

It has now been shown that coal occurs in seams of workable thickness beneath Apley Barn, and the quality of these seams is being studied by officers of the National Coal Board. Thus the name Oxfordshire Coalfield gains the significance which it lacked when E. N. Arber was criticized for coining the name in 1916 (ref. 14), on the evidence of the Signet Burford borehole, where no coal of workable thickness had been found.

The economic potentialities of the coalfield are as yet unproved. The completion of its exploration would be a lengthy procedure which is not the responsibility of the Geological Survey of Great Britain, though that organization would give geological advice if requested. A series of further boreholes would be required, and if these proved good-quality coal seams at a workable depth over a sufficiently wide area, then only might the need arise for the siting of a colliery shaft. Such a site would most probably be chosen near a railway line and Oxfordshire, like Kent, would have its concealed coalfield.

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THE TRAINING OF BIOCHEMISTS

UNTIL comparatively recently most biochemists in Great Britain had received their early training in other disciplines such as chemistry, physiology or medicine. However, the rapid progress of biochemistry during the post-war period has been paralleled by an increase in the number of biochemistry departments within British universities, and it is likely that most of our future research biochemists will graduate from these departments, having followed courses leading to the honours B.Sc. degree.

It is probably true to say that no one is yet certain, to the point of dogmatism, as to the best way of training such biochemists, and this no doubt reflects the position of biochemistry as a 'meeting-point' of so many other disciplines. In order to create discussion and thought on this topic the Biochemical Society held, during the afternoon of July 13, a colloquium on "The Training of Biochemists". The colloquium, which formed part of the four-hundred and eighth meeting of the Society, was held in Oxford under the chairmanship of Dr. K. S. Dodgson and was attended by more than 250 members and guests.

One of the principal controversies of the proceedings centred around the contributions of the first two speakers. Prof. S. V. Perry outlined the growth and development of biochemistry in Britain and stressed the value of the three- or four-year B.Sc. honours course in the training of the biochemist. Such courses were now firmly established in their own right and provided not only an intellectual challenge but also a sound basis for education in its broader sense. In contrast, Prof. B. Spencer suggested that biochemistry was more fitted to be a postgraduate subject, and illustrated his point with detailed reference to the systems in use in most American universities. Some lively discussion followed these contributions and although the weight of opinion was perhaps loaded in favour of the undergraduate course it was clear that most people, recognizing the important links between biochemistry and other disciplines, felt that other pathways by which biochemists emerged should remain open. Prof. R. A. Morton reiterated from the floor that the educational aspects of a university course should not be neglected in favour of 'training'. Biochemistry as an undergraduate subject was intellectually stimulating, and courses should always include aspects hard enough to stretch the mind of the student. What matters is the spirit of the course rather than the factual content.

The views of industry regarding the training of biochemists were outlined by Dr. W. F. J. Cuthbertson, who pointed out that industry as a whole was still not fully aware of the potential value of the trained biochemist. He suggested that a group should be formed, composed of representatives from academic life, industry and the various industrial research associations, to determine the contribution that biochemical graduates could make in industry, their present level of employment and the possible future requirements for them. In noting the differences between research in industry and in universities or research institutes, Dr. Cuthbertson suggested that industrial research was in some ways more exciting and variable. The freedom of academic research was sometimes illusory, and the academic research worker soon realizes that certain projects and methods of approach are more likely than others to attract financial aid to research. Most large industrial concerns exhibit a liberal attitude towards research, and the industrial biochemist is usually free to work on anything he choses providing that he can persuade his authorities to supply the necessary facilities. The training that was appropriate to the industrial biochemist was not markedly different from that followed by most university departments, but additional attention might be given to special facets such as microbiology and the use of statistics. The ability to present an account of his work in unambiguous simple English was perhaps even more important for the industrial biochemist than for his academic friends, because many of his findings may have to be assessed by people with little or no training in his own discipline or, for that matter, in any other scientific subject.

After some preliminary sparring with questioners, Dr. Cuthbertson was challenged outright as to whether industry was satisfied with its biochemical recruits. In reply he said that, generally speaking, expanding university departments and research institutes were skimming the cream of the graduates and a more encouraging attitude to industry from within university departments would be welcomed. Several speakers suggested that industry was perhaps responsible for any unfavourable attitude which might exist and, in particular, the use of graduates as 'superior technicians' by industry was deprecated. This unwelcome development certainly appears to be on the increase, and reflects the present shortage of trained technical assistants—a situation which technical colleges in Britain should be given every encouragement to ameliorate.

University and departmental entrance requirements were discussed by Dr. Margaret Kerly, and the meeting was greatly indebted to her for the work she had done in collating so much diverse and scattered information. In biochemistry, perhaps more than any other subject, opinions as to the school background appropriate to students vary considerably. Dr. Kerly pointed out that the number of sixth-form students from any one school who were interested in a biochemical career varied from none to 2 or 3 only, and it was therefore unrealistic to expect schools to modify time-tables in order to cater for potential biochemists. Time-table problems usually resulted in sixth-formers taking chemistry, physics and mathematics for advanced level General Certificate of Education, in which case they were weak on the biological side, or alternatively, chemistry, physics or mathematics and a biological subject, in which case they tended to be weak in either physics or mathematics. A possible solution might be the introduction of a new advanced level subject which combined those sections of the present physics and mathematics courses which are most necessary for all biologists.

In spite of the Biochemical Society's recent pamphlet, *Careers in Biochemistry*, inquiries in schools had convinced Dr. Kerly that many science masters and sixth-form students were only vaguely aware of the potentialities of a career in biochemistry. More propaganda will be needed in schools if more good candidates for biochemistry are to be forthcoming. Further support for this contention came during

discussion, and it was suggested that individual departments could and should do more in this respect, particularly in the light of the shortage of biochemists which Britain is now experiencing.

In describing the recently revised four-year biochemistry course at Oxford, Prof. D. D. Woods stressed the importance of adequate background teaching, particularly in chemistry, but pointed out the need to introduce biochemistry at an early stage in the course in order to stimulate student interest. Experimentation in biochemical research is steadily becoming more sophisticated, and recent trends have emphasized the more biological aspects of the subject. The early introduction of such topics as cell physiology and genetics is therefore an important feature of the course at Oxford, and the Biochemistry Department was particularly fortunate in having the co-operation of other departments in the teaching of such topics.

Much interest was expressed in the course at Oxford, particularly from the point of view of its breadth of outlook, although it was clear that Oxford with its resources was in a particularly favourable position to overcome time-table problems which so beset smaller departments.

In the final paper, Dr. E. M. Crook discussed the M.Sc. course in biochemistry which has been in existence for some years at University College, London. Dr. Crook suggested that a good deal can be said for not attempting to teach biochemistry as an undergraduate subject, but a major disadvantage in adopting this attitude, so far as the course at University College, London, had been concerned, was that few M.Sc. candidates have learned any biochemistry, even of the most elementary kind, before entering the course. Moreover, a first-class honours graduate in, for example, chemistry has to be exceptionally strong-minded to proceed to an M.Sc. course in another subject in the face of the psychological impulse to go on at once to prove himself as a research worker. The danger, therefore, is that the M.Sc. course may tend to attract the less-able student who may look on the course as a means of 'upgrading' a relatively poor initial degree. On the whole, however, it seems likely that postgraduate teaching must increase. Various departments could, between them, provide a formidable series of postgraduate courses of different specialist nature, and this could prove of great value in strengthening the subject in Britain.

In summarizing the proceedings, Prof. F. C. Happold confessed himself to be out of sympathy with the title of the colloquium. We were not concerned with the training of biochemists as one trains circus animals but with their education. The latter embraces the former but it is also more difficult to achieve. In any event, those departments which already had honours courses in biochemistry were certainly not proposing to abandon them. Such courses were an excellent education in themselves providing that sufficient time was available to do the job properly. In his own opinion the four-year course appeared to be particularly satisfactory from this point of view.

Judging from the conversations which followed the closing of the meeting, the original aims of the colloquium in stimulating thought and discussion had been successfully achieved. The Biochemical Society hopes to publish the proceedings of the meeting, in monograph form, at an early date.

K. S. Dodgson

NEWS and VIEWS

New President of the British Association: Sir John Cockcroft, O.M., K.C.B., C.B.E., F.R.S.

Sir John Cockcroft, Master of Churchill College, Cambridge, and part-time member of the U.K. Atomic Energy Authority, has been elected president for 1962 of the British Association for the Advancement of Science. That year, the annual meeting will be held in Manchester.

As is well known, Sir John has achieved world-wide fame as an atomic physicist. He was Jacksonian professor of natural philosophy in the University of Cambridge during 1939–46. His first researches, beginning in 1924 at Cambridge, were concerned with the heating of transformer coils, and later, at the Cavendish Laboratory, he carried out work on the deposition of surface films by atomic beams. Later, with Kapitza, he designed much of the equipment used in the production of intense magnetic fields. In 1932, with Walton, he had devised the arrangement by which nuclear disintegration by artificially accelerated particles was first observed. He was a prime mover in the development of equipment and the erection of buildings for which the physicists at Cambridge must now be grateful. During most of the Second World War period he was chief superintendent of air defence, Research and Development Establishment, Ministry of Supply, and then became director of the Atomic Energy Division, National Research Council of Canada, during 1944–46. He was then appointed director of the Atomic Energy Research Establishment at Harwell.

Such is the high esteem in which his work is held that Sir John has been or is a member of many scientific and advisory organizations and he has been the recipient of numerous State, academic and other honours. Among the most recent are the 1961 Atoms for Peace Award, which was established as a memorial to Henry Ford and his son Edsel, consisting of a gold medal and 75,000 dollars, and the chancellorship of the Australian National University, Canberra.

Norwich and its Region

Visitors to Norwich will find *Norwich and its Region* a very pleasing work. (Pp. 224. Norwich: Local Executive Committee of the British Association for the Advancement of Science, 1961. 15*s*.). It provides a most informative survey of the region of Norfolk and north Suffolk. The survey opens with a concise account of the historical events which have moulded East Anglia. To-day it is in the midst of a second industrial and agricultural revolution. Though to some it may appear a quiet backwater, residents are conscious of the mounting pace at which it is being drawn relentlessly into the main stream. The second section deals with geology, physiographical features, climate, and the nature of the soil. Results of recent research are emphasized. In fact, advances in knowledge of the Pleistocene deposits have been

so rapid during the past few years that the authors feel obliged to point to the interim character of the account they give. The biological background of the area provides the theme for another section. Despite the extent of agriculture, much natural vegetation is still preserved. The county is actively concerned with conservation of Nature. About three-quarters of the British flora abound in Norfolk, due to the climatic contrast coupled with differences in soils. The county is rich in non-marine Mollusca; the coast, however, provides little shelter for marine animals.

The fourth theme deals with the influence of man on the environment. Physical anthropology, archæology and social, political and religious history are discussed. East Anglians are proud to be individualists, ever loath to follow the accepted pattern. From very early times, social services were instituted for the care of the sick and poor, and opportunity for schooling has not lagged behind that in the rest of England. Norfolk has a record of independence and enterprise in education no less than in farming. The final section gives a detailed informative account of the economy of the area to-day. Primary industry, like agriculture, forestry and fishing, is discussed; so too is the growth of manufacturing industry and commercial and professional services. The authors of these surveys are to be congratulated, since their natural emotional involvement with the various topics is never allowed to distort their scientific objectivity. The photographs are numerous and excellent. There is a very good map of the region. The photograph on the back cover is exquisite—a golden splendour of barley. That the book has been sent to members in advance of the meeting is to be welcomed.

Anatomy at Oxford : Sir Wilfrid Le Gros Clark, F.R.S.

Sir Wilfrid Le Gros Clark retires from the chair of anatomy in the University of Oxford at the end of the next academic year. He is probably the last in the line of the great medical men who have been able to combine a career in anthropology, in which he is among the most distinguished of its leaders, with that of an equally distinguished career in neuro-anatomy. His classical work on the nuclei and connexions of the primate thalamus, his work on the visual and olfactory systems and many other parts of the brain have shown that he is equally as good at noting the morphological changes which follow controlled experimental procedures as in descriptive anatomy. His book, *The Tissues of the Body*, is a masterpiece of lucid writing and probably the best introduction to human biology which has yet been written. In this book he shows that anatomy is the science concerned with the organization of the living body and how this is achieved during embryonic, post-natal and adult life. It must embrace the whole range of organic structure, the organization of the elementary components of living matter into cells, cells into tissues, tissues into organs, organs into systems, and systems into integrated creatures. He, like Woollard before him, urged that the teaching and study of minutiæ of topographical anatomy are an unjustifiable burden to those training to become doctors, for nowadays no potential physician, surgeon or clinical scientist can afford the time to master more than a few of the details offered by any one of the basic medical sciences, least of all those of no intellectual or even practical value. This is not to deny, however, that anatomical details are of great importance to surgical specialists, but it is by them that they should, and can most easily, be taught.

Prof. G. W. Harris, F.R.S.

Sir Wilfrid's successor will be Prof. G. W. Harris, the distinguished neuro-endocrinologist, who is at present Fitzmary professor of physiology in the Institute of Psychiatry in the University of London. Prof. Harris started his professional career in anatomy at Cambridge, where he taught for eight years and where he laid the foundations of his endocrinological research work. The electors, in choosing Prof. Harris, have shown boldness and imagination. Although, at present, he is by title a professor of physiology, he should make a most worthy successor to Sir Wilfrid, for he, like Sir Wilfrid, is interested in structural organization. For example, he has demonstrated in a series of elegant experiments that the pituitary gland (adeno-hypophysis) only operates normally when it lies in the pituitary fossa. It is not sufficient that the gland, when transplanted elsewhere in the body, remains structurally intact. Only when it is returned to the site in which it originally developed (that is, lying close to and having direct vascular relationships with the hypothalamus) does it again function normally. This work is a perfect demonstration of the importance of structural organization in relation to functional requirements, and alone suffices to indicate that Prof. Harris is well suited to develop the science of anatomy at Oxford, the foundations of which have been so well laid by Sir Wilfrid. An appointment of this kind underlines the current feeling in medical educational circles that instruction in any form of detailed topographical anatomy will have to pass into the hands of surgical specialists, as indeed it already has in some medical schools.

Visit of Russian Scientists to Harwell

As a result of the agreement recently signed between the U.K. Atomic Energy Authority and the Russian State Committee for Utilization of Atomic Energy for Peaceful Purposes, a party of five Russian scientists visited England during July 20–30 to discuss fundamental solid state physics. In order to allow the Russians to see a good cross-section of British work, the Atomic Energy Authority invited a number of university scientists to attend discussions at Harwell on radiation damage, electronic and magnetic properties of solids, and on neutron diffraction. The party also visited physics and metallurgical laboratories of the Universities of Reading, Oxford and Cambridge. The visit afforded an opportunity of learning a little about the nature and scope of work carried out at some of the Russian research institutes. It is clear that substantial results in the fields of inelastic neutron diffraction and electron microscopy of thin films may be expected in the next few years, following a marked expansion in Russian efforts in these topics which started in 1959. The Russian party was led by Prof. V. E. Ivanov, of the Physico-Technical Institute of the Ukrainian Academy of Sciences, whose main interest lies in the field of purification of metals, and included Prof. N. F. Pravdyuk, of the Kurchatov Atomic Energy Institute, Moscow, who spoke on radiation damage; Mr. V. I. Bykov, of the Obninsk Physico-Energetics Institute, who spoke on neutron diffraction; Mr. B. B. Baturov, of the State Committee for Utilization of Atomic Energy, whose main interest is in nuclear reactors; and Mr. A. D.

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Sokolov, of the State Committee for Utilization of Atomic Energy, whose main interest is in particle detectors and instrumentation for physics experiments.

U.S. Nuclear Power Projects

The third edition of *Fact Sheets on U.S. Nuclear Power Projects* is a complete revision of the sheets first published in December 1958 and revised in June 1960 (Pp. iii+51. New York: Bozell and Jacobs, Inc., 230 Park Avenue, 1961). The reactors listed are classified by coolant. The water-cooled reactors include five pressurized and eight boiling-water reactors. The sodium-cooled reactors comprise two moderated and two fast-breeder reactors, and one organic-cooled and three gas-cooled reactors are also listed. The introductory article discusses the approach to nuclear power development in the United States which has not been, and is not, aimed primarily at producing kilowatts, but is designed to develop technical information for the construction of commercial plants by the electrical utility industry. At least 133 investor-owned electricity companies in the United States of America are actively engaged in civilian nuclear power projects, including plants in operation; under construction, or under contract, or planned; or in study, research, and development projects; a complete alphabetical reference list of the projects is given at the end of the *Fact Sheets*.

New Hope for African Wildlife

Great concern has been felt in recent years about the accelerating rate of destruction of wild fauna, flora and habitat in Africa, without regard to their value as a continuing economic, scientific and cultural resource. It is feared that by 1970 several of the largest land mammals in Africa will not survive in a wild state. The smaller wild creatures are already under constant threat as the New Africa expands and demands on its natural resources grow. At the 1960 Warsaw Congress of the International Union for the Conservation of Nature it was recognized that the crux of the problem is to persuade the people of the New Africa to appreciate the value of their wild life not only in relation to the growth and prosperity of their country but also for the sake of future generations. It was at this Congress that the African Special Project was planned in three stages: (1) survey of problems; (2) conference to discuss possible solutions; (3) operational work to implement the solutions. Stage 1, which has already been completed, consisted of a survey carried out in sixteen African countries. Stage 2 has been planned and prepared in detail. The conference on "The Conservation of Nature and Natural Resources in Modern African States" will be held in Arusha, Tanganyika, during September 5–12. It is organized jointly by the International Union with the Commission for Technical Co-operation in Africa. For practical reasons the number of delegates had to be limited, but will include representatives of 25 governments. A number of 'fellowships' has been provided so that selected Africans of the younger generation can participate. Further information can be obtained from the Press Officer to the Arusha Conference, Ian S. MacPhail, 20 Belgrave Square, London, S.W.1.

Recent Map Publications by the Ordnance Survey of Great Britain

On August 1, the last six sheets were published by the Ordnance Survey of Great Britain, completing the Seventh Series of the One Inch to One Mile Maps, and for the first time the whole of Great Britain is covered by a single series of maps at this scale, in which all sheets are similar in projection, size and style. The six sheets are: Sheet 1, *Shetland Islands* (*Yell and Unst*); Sheet 2, *Shetland Islands* (*North Mainland*); Sheet 3, *Shetland Islands* (*Lerwick*); Sheet 4: *Shetland Islands* (*South Mainland*); Sheet 188, *Torquay*; Sheet 190, *Truro and Falmouth*. At the same time, six publications of the new post-war series of One Inch Tourist Sheets have been published: *North York Moors; Peak District; Wye Valley and Lower Severn; Lake District; Lorn and Lochaber; Loch Lomond and the Trossachs*. With the exception of the sheet *Wye Valley and Lower Severn*, where relief is depicted by contours and layer tints, the sheets in this series show relief by various combinations of layer tints and hill shading, which give an immediate impression of ground shapes even to the inexperienced map reader. In other respects, the style remains generally the same as that used for the One Inch Seventh Series. Copies of all sheets are available in three styles from all Ordnance Survey agents and H.M. Stationery Office bookshops (Tourist Sheets: paper flat, 5*s*.; paper folded in covers, 7*s*.; mounted on linen in covers, 9*s*. Seventh Series: paper flat, 4*s*. 6*d*.; paper folded in covers, 5*s*. 6*d*.; mounted on linen in covers, 7*s*. 6*d*.).

Problems of Psychology

A new quarterly journal (*Problems of Psychology*, Nos. 1 and 2. Pergamon Press, 1960) aims at publishing a selection of papers in translation from *Voprosy Psikhologii*, the Russian psychological journal founded in 1955. Dr. N. O'Connor, who is acting as translation editor, states in his foreword that he hopes to choose papers which will be both representative of Soviet psychology and at the same time of special interest to English readers. The present issue contains papers on a wide variety of topics, ranging from the role of the cerebral cortex in the perception of flicker to the psychology of a man with prodigious powers of memory. Of particular interest to English psychologists is Dr. E. N. Sokolov's extremely ingenious probability model of tactile perception. Although rather uneven, this new psychological journal will be widely welcomed.

The Royal Scottish Museum

The annual report of the Royal Scottish Museum (Pp. 29. Edinburgh: Royal Scottish Museum, 1961). Edinburgh, for 1960 will be the last to appear under the control of the present director, Dr. D. A. Allan, for he retires shortly (see *Nature*, August 12, p. 648). The report indicates progress in all directions especially as regards the carefully planned building operations. The lecture-theatre block approaches completion, fluorescent lighting has been installed in the store rooms, and a further series of exhibition galleries have been redecorated in what is now regarded as the standard colour scheme. Considerable progress has also been made on paper with the replanning of the north-east and north-west wings facing Chambers Street. The introduction of automatic stoking and thermostatic control has led to difficulties in securing adequate humidity, but further experiments to remedy this are being carried out. The consequent diminution in work, dust and dirt in the basement storage is marked and welcomed. As usual, temporary exhibitions have been a valuable feature in the activities of this Museum, and a total annual attend-

ance of nearly 357,000 testifies to the place of this institution in the life of the community and with visitors to the City. The director and his staff continue their valuable connexions with other cultural bodies, not only nationally but also internationally, and Dr. Allan has reason to be well satisfied with the present state of the Museum which he has directed with such conspicuous success.

Aids to Agriculture and Horticulture in Northern Ireland

THE Ministry of Agriculture in the Government of Northern Ireland has produced a series of leaflets giving advice to those concerned with the raising of crops and animals. The leaflets are practical, clearly written and would be of assistance to nurserymen, gardeners and farmers managing large or small farms. One hundred and thirty-eight leaflets have already been issued dealing with topics as varied as "Liver Fluke Disease", "Results of Potato Variety Trials", "The Feeding of Bacon Pigs", "Turkey Production", "The Cultivation of Gooseberries" and the "Construction of Farm Dairying Buildings". The leaflets may be obtained free of charge from the Ministry of Agriculture, Stormont, Belfast.

The Plastics Institute

SIR LAURENCE MERRIAM, chairman of British Xylonite since 1959, has been elected president of the Plastics Institute for the session 1961–62. The following have been elected vice-presidents of the Institute for 1961–62: P. C. Chaumeton, Sir John Dean, P. A. Delafield, G. Dring, S. Ellice-Clark, G. W. Hodds, J. Lesser, W. F. Mitchell, A. E. Skan, Sir Miles Thomas, W. C. Waghorne, G. J. Wevell. The new ordinary members of Council for 1961–62 are: Dr. F. P. Hiron, M. Jones, M. Kaufman, A. A. Tomkins.

Fleming Memorial Fund

THE Trustees of the Fleming Memorial Fund for Medical Research announce that the first grants from the Fund will be made before the end of 1961. The Fund exists to commemorate Sir Alexander Fleming, and his discovery of penicillin, by raising money to provide assistance for basic medical research anywhere in the world. This assistance may be given by grants to universities, research institutions and others engaged in basic medical research, and by way of national and international fellowships tenable at universities and research institutions. Applications should be sent to the Secretary to the Trustees, Fleming Memorial Fund for Medical Research, 26 Upper Brook Street, London, W.1, on or after October 1, 1961.

Announcements

MR. F. THISTLETHWAITE, lecturer in the Department of Economics and Politics, University of Cambridge, has been appointed the first vice-chancellor of the University of East Anglia, Norwich.

THREE meetings on "Nutrition in Latin America", organized by the Food and Agriculture Organization, will be held during the autumn: *September 27–October 7*, nutrition seminar for the Caribbean (Puerto Rico); *October 11–21*, seminar on nutrition education for Mexico and Central America (Guanajuato); *October 23–November 15*, joint symposium with the World Health Organization (Guatemala City). Further information can be obtained from the Food and Agriculture Organization, Viale delle Terme di Caracalla, Rome.

THE eleventh annual instrument symposium and research equipment exhibition, sponsored by the Washington Sections of the American Association of Clinical Chemists, American Chemical Society, Instrument Society of America, Society of American Bacteriologists, Society of Applied Spectroscopy, Society for Experimental Biology and Medicine, will be held at the National Institutes of Health in Bethesda, Maryland, during October 9–13. Further information can be obtained from J. B. Davis, National Institutes of Health, Bethesda 14, Maryland.

THE twenty-second national conference of the Council for the Preservation of Rural England is to be held at Southport during October 12–14. The subject of the conference, which is open to all, will be "The Impact of Industrial Development on the Countryside". Further information can be obtained from the general secretary, Sir Herbert Griffin, 4 Hobart Place, London, S.W.1.

THE first European symposium on "Food Technology" is now to be held in Frankfurt (Main) during October 26–27, instead of October 19–20 as previously announced. Further information can be obtained from Prof. W. Diemair, Georg-Voigt-Strasse 16, Frankfurt (Main).

AN international meeting on "Psychophysiology, Neuropharmacology and Biochemistry of Audiogenic Seizure" is to be held by the French National Centre for Scientific Research during November 6–9. Further information can be obtained from Prof. R. G. Busnel, Laboratoire de Physiologie Acoustique, C.N.R.Z., Jouy-en-Josas (Seine et Oise), France.

THE International Atomic Energy Agency, the Food and Agriculture Organization and the World Health Organization are jointly organizing a conference on "The Use of Radioisotopes in Animal Biology and the Medical Sciences" in Mexico City during November 21–December 1. Further information can be obtained from the Joint Secretariat, Conference on the Use of Radioisotopes in Animal Biology and Medical Sciences, International Atomic Energy Agency, Kaerntnerring 11, Vienna.

LIST No. 29, "Forthcoming International Scientific and Technical Conferences", issued by the Department of Scientific and Industrial Research, amends and amplifies the information contained in List No. 28, which is superseded, from July 1961 to December 1962. As is the usual practice, conferences have been given chronologically and an index added to facilitate reference. The list is obtainable from the Department of Scientific and Industrial Research, Charles House, 5–11 Regent Street, London, S.W.1.

THE fourth edition, revised to November 1960, of *A Bibliography of African Bibliographies, covering Territories South of the Sahara*, issued by the South African Public Library, Cape Town, includes some 1,450 entries, more than 350 of which are new, arranged according to the Universal Decimal Classification, with somewhat more intensive geographical sub-division (Grey Bibliographies, No. 7. Pp. iv + 79. (Cape Town: South African Public Library, 1961.) R. 2.00; 20*s*.). There is also an author-subject index combined.

XUM

SPACE-FLIGHT AND RE-ENTRY TRAJECTORIES

A SYMPOSIUM on "Space-Flight and Re-entry Trajectories", organized by the International Academy of Astronautics, was held at Louveciennes, near Paris, during June 19–21, 1961. The conference was opened by Prof. Theodore von Kármán, who has chosen to follow up his long and distinguished career in aeronautics by becoming the first director of the Academy. This symposium constituted the first meeting of the Academy, and the subject of trajectories was chosen, Prof. von Kármán said, because of its fundamental importance in the successful development of space flight. Though the Academy was now only beginning its activities, Prof. von Kármán was confident that in perhaps 100 years it would be as 'respectable' as the Academy of Sciences or the Royal Society. The present situation of astronautics reminded him of that of aeronautics when he first came to France in 1908. Then, aeronauts were regarded almost as madmen, and no one truly foresaw the later commercial and military applications of aircraft. Even the pioneer Lanchester was worried by the cost of upkeep of aerodromes, and thought they would have to be divided into two parts, to be grazed by cows in alternate years. With so many false aeronautical prophecies among his memories, Prof. von Kármán was confident of the future of astronautics.

The first scientific session, on "Trajectories for Lunar and Interplanetary Missions", began with a paper by J. M. J. Kooy (Breda, Netherlands) on "Astrodynamics and Planetary Research", which investigated the motion and uses of space-vehicles placed in orbit about a planet such as Venus. Then H. Hiller (Royal Aircraft Establishment, England) described the results of a generalized study of two-dimensional trajectories in the region of the Earth and Moon, and W. E. Moeckel (U.S. National Aeronautics and Space Administration) discussed interplanetary trajectories for electrically propelled space vehicles.

The second session, on "Orbital Transfer and Rendezvous", comprised three papers on the optimization of interplanetary trajectories, by D. F. Lawden (Canterbury, New Zealand), P. Contensou (Office National d'Etudes et de Recherches Aéronautiques, France) and B. Fraeijs de Veubeke (Liège, Belgium), and a general study of the techniques of orbital rendezvous by J. C. Houbolt (U.S. National Aeronautics and Space Administration). These subjects promise to expand greatly in the future, since both are essential to the economical performance of space missions and both pose severe mathematical problems.

In the third session, on "Near Earth Satellites", J. Kovalevsky (Bureau des Longitudes, Paris) made a general analytical review of orbital perturbations, D. G. King-Hele (Royal Aircraft Establishment, England) discussed the effect of atmospheric oblateness on satellite orbits, L. Sehnal (Ondrejov, Czechoslovakia) described luni-solar perturbations on a 24-hr. satellite, and K. Schütte (Munich) commented on the changes in the orbit of *Echo* 1.

The final session, on "Dynamics of Terminal Re-entry", was, ironically, delayed because the coach conveying the participants pursued a determinedly non-optimum terrestrial path from Paris to Louveciennes and took nearly 2 hr. to complete a journey nominally of 10 miles. In this session, A. Ferri and T. Ling (Brooklyn, U.S.A.) described the effect of varying lift on re-entry trajectories, W. F. Hilton (Hawker Siddeley, England) considered re-entry paths which avoid the zones of radiation, and errors in re-entry guidance were discussed by T. R. F. Nonweiler (Queen's University, Belfast) and L. Broglio (Rome). Finally, J. V. Becker, D. L. Baradell and E. B. Pritchard (U.S. National Aeronautics and Space Administration) investigated the methods of control for re-entry from escape speeds.

Although some of the papers seemed to be slightly out of touch with the present realities of space research, this was perhaps appropriate, or even inevitable, in a forward-looking academic symposium. Certainly the symposium as a whole can be counted a success, and the International Academy of Astronautics was wise to limit the numbers of those attending to about 80. Representatives from the U.S.S.R. were invited, but were unable to come. The papers presented at the symposium are to be published in *Astronautica Acta* later this year.

D. G. King-Hele

BIOLOGY IN BRITAIN

By Dr. H. V. WYATT

Department of Bacteriology, University of Leeds

SINCE the War, more and more exciting discoveries have been made in biology, but the classical biology of pre-war years has changed little. New techniques and brilliant men have opened new and fundamental subjects of inquiry, which cannot be wholly contained by the separated disciplines of botany and zoology but have found equal application in both subjects. Perhaps for the first time there is now a subject that can be called 'biology'.

A British biologist visiting the United States has at least two shocks in store for him; the first is the breadth of interest of his American colleagues and the second is the scope of the American undergraduate programme. In the United States there are departments in biology where the staff may include an ecologist, a physiologist, a biochemist, a microbial geneticist, a plant pathologist and perhaps an embryologist. An undergraduate may study zoology, botany, biochemistry, microbiology and genetics as well as chemistry. Afterwards, if he undertakes postgraduate studies, he will attend courses in an even greater variety of subjects. We may be doubtful and scoff 'superficial'

and 'multiple-choice', and there are certainly faults in the American educational system, but the American biology graduate and Ph.D. is better equipped than the British counterpart, better equipped to teach other subjects, better equipped to read the journals, better equipped to do and to direct research. The advances that are being made are the results of the application of the techniques of one subject in others. Embryology is now studied by using techniques of biochemistry and immunology ; major advances have been made in genetics by the use of micro-organisms; micro-organisms have contributed to our understanding of biochemistry and now the biochemistry of deoxyribonucleic acid, the chemical basis of heredity, is being studied by using the viruses that infect bacteria. The artificial barriers are down: only the complete biologist can hope to map the new territories, although all the time the discovery of new landmarks makes it easier to find the way.

The emphasis in teaching is on the vertical division: animals and plants are catalogued in an ascending scale of morphological complexity corresponding to the natural evolution of the groups. But British practice in research is based increasingly on a horizontal structure; people who work on the active transport of inorganic ions and organic molecules use yeast, bacteria, plant roots, parasitic worms, mammalian red blood cells and so on. The fundamental properties of active transport are probably the same in all cells and can be studied in any convenient system; methods of studying it are peculiar to active transport and do not depend on the type of cell. From these studies we may discover whether cells of different origin behave in the same way or whether those differences we find have an evolutionary or an ecological significance. Already we have a pragmatic subject, microbiology, in which unity derives from common techniques and not from an evolutionary concept of a common ancestor. Research emphasizes the unity of modern biology; our teaching must do the same, for only by teaching biology as a unified discipline can we hope to train biologists adequately. It is essential that while we teach the latest findings of biochemistry and biophysics, we also give our students a love and understanding of living plants and animals. If the biologist is taught little biochemistry, the biochemist lacks contact with the living.

Many subjects are taught in isolation when their integration would make plain their coherent pattern. Recent experiments have shown that diet and behaviour may influence the interaction of parasite and host; behaviour is affected by the hormone balance of the animal, and this in turn is influenced by the animal's environment. We must frame our courses so that the student is made aware of such complex interactions. To do this we must persuade the staffs of our universities from the isolation of their departments. A degree of trade union demarcation has begun in our subjects; we now specialize so early that there is a tendency for the student with a first degree in microbiology, for example, to be restricted to that subject; although his techniques could be used there to advantage, he is rarely encouraged to apply for a job in a zoology department.

If the new biology is experimental and not in the first place descriptive, on what shall we model our course ? At least one course that we could copy is already in existence. For the Diploma of Technology in applied biology at the colleges of technology the emphasis after the first year is on physiology, biochemistry, pharmacology and microbiology. Elementary mathematics, statistics and organic and physical chemistry are also compulsory subjects. It is significant that these courses were planned by departments of biology in colleges where there had been no fragmentation into minor subjects. Although these courses have been planned with the view of the needs of industry, they are more suited for academic biology and research than are the courses offered at the universities. Sixth-form students may well think so too, as they read the topics and experience required in the "Situations Vacant" columns. Already our students may be learning more about modern biology from the *New Scientist*, the Penguin Science Survey and television than they gain from the isolated subjects studied at a university.

It is important that, in presenting the mosaic to show both the pattern and the basic units, we do not leave the student either bewildered or complacent. Bewildered if, faced with two thousand references about a single animal, he cannot imagine what more can be discovered or where to start; complacent, if he is presented with knowledge as a set of facts and theories and techniques as unquestionable. Both might be avoided by a detailed observation that would give competence with several techniques, an appreciation of the complexity of an organism and, most of all, an awareness that science is not dormant but militant. One evident way to attempt this would be to give a class several strains of a bacterium. Each week the class would study their own cultured strains in a progressive series of experiments. These could include growth, nutritional requirements, antibiotic sensitivity, genetics and phage-susceptibility —each of which contributes to the understanding of the others. If we base our undergraduate courses on modern experimental techniques with wide application, we can shed some of the crafts which now occupy so much time in school and university. As we do so, we may attract students who now take the physical sciences.

As soon as possible we must bring all the biological sciences together in one building. This is necessary not only so that the staff may feel that they are all biologists but in order that courses can be integrated. Staff using the same or similar techniques will be encouraged to collaborate and form new loyalties. Apparatus can be shared for both teaching and research. Physical and financial separation now makes this difficult. With the present departmental system, expensive apparatus is bought by one department, used, and afterwards lies idle. In another department, similar equipment is bought from another source. While we lack in the universities much modern equipment necessary for research, we continue to buy two pieces of apparatus where one would suffice.

A director of the National Science Foundation, which is revolutionizing the teaching of science in the United States, has written of the need for more experimental biology in Europe[1]. In Britain recently there has been argument for the creation of more chairs of biochemistry and molecular biology or biophysics. The need for such chairs is urgent: Prof. Irene Manton, in a recent broadcast, suggested that more chairs be created within existing departments; and, I may add, we need fusion of the biological sciences for teaching and research. Within a unified department of biology, it will be easier to add new disciplines than it is, nowadays, to create new departments.

[1] Consolazio, W. V., *Science*, **133**, 1892 (1961).

RESEARCH WORK OF THE NATURE CONSERVANCY

AN adjournment debate in the House of Commons on July 21, at which was discussed the research work of the Nature Conservancy, was opened by Mr. M. Kimball with an attack on the work of the Conservancy which showed a complete misunderstanding of the purposes and nature of the Conservancy. Nevertheless, the debate as a whole should prove a useful piece of publicity for the work of the Conservancy, which still lacks the resources necessary for an adequate publicity policy to remove misunderstandings such as those exemplified by Mr. Kimball's speech. On the matter of finance, it may be noted that, although several speakers commented on the inadequacy of the Conservancy's budget of no more than £500,000 a year, the Parliamentary Secretary for Science, Mr. D. Freeth, made no comment beyond saying that it is a matter for individual judgment whether the Conservancy's resources are too small.

Mr. Freeth, recalling the Director-General's description of the research programme as "a comprehensive attack on a range of ecological problems which are regarded as of fundamental significance for the advance of conservation", maintained that besides increasing scientific knowledge, the Conservancy's work is in the long run of great benefit to the nation. Of a staff of 250, 100 are scientists, and the Conservancy also maintains 40 postgraduate students working for doctorates in ecology and related subjects. It is, he insisted, a research council, and although it does its best to encourage interest in Nature and its conservation, this is not a primary purpose, and one has to recognize that many of the reserves are established to be sanctuaries for wild life, a purpose inconsistent with free access. The Conservancy welcomes at all its reserves scientists and university workers who have research to do which is likely to be of value. Mr. Freeth does not believe that there is overlapping with the work of other Government research organizations. There is considerable interlocking of personnel at a very high level, as well as on *ad hoc* committees, and the Select Committee on Estimates which explored the question of relations with the Forestry Commission in 1958 found no evidence to justify a recommendation to transfer to the Commission some of the fundamental research now undertaken by the Conservancy. Much of the Conservancy's research is basic in character and not suitable for dissemination through the National Agricultural Advisory Service.

The Conservancy, Mr. Freeth pointed out, controls about 8,000 acres of woodland, and research here is essential: first, to provide representative examples of the more important ecological types in their earlier stages of development; secondly, to provide adequate areas of managed woodland which would otherwise be threatened by systems of management more concerned with profit-making; and thirdly, to provide specialized facilities for undisturbed research and long-term field-trials. Mr. Freeth specifically denied the suggestion that the Nature Conservancy has ever established the breeding of coypu. For publicity or educational campaigns, or the issue and enforcement of regulations, the Conservancy has to look, for example, to the Ministry of Agriculture or of Transport.

As regards liaison and co-operation with county naturalist trusts and other bodies of naturalists, Mr. Freeth pointed out that the Select Committee on Estimates was impressed with the argument that unique opportunities for research would be lost if the Conservancy did not acquire as soon as possible the whole of the 250,000 acres envisaged. At present, 88 reserves represent 177,000 acres, and Mr. Freeth does not think it reasonable to delay proceedings merely because in a particular area a county organization has not been formed. He welcomed Mr. Kimball's praise of the Nature Conservation Corps, but to what was perhaps Mr. Kimball's most important point—that for only half the reserves has a five-year management plan been established—he made no reply. If this is due simply to financial stringency, it is time that the Minister for Science—and Parliament—acted.

SALMON AND FRESHWATER FISHING IN GREAT BRITAIN

RECENTLY the Committee on Salmon and Freshwater Fisheries produced a stimulating report in which are made several recommendations which may lead to some very necessary changes in freshwater fishery legislation.*

Of major importance are the recommendations that the powers of the River Boards be extended. The report rightly appreciates that fishery problems are often localized and suggests that River Boards should be allowed to make by-laws to suit local conditions. Equally important are the recommendations that River Boards should have the power to demand accurate returns of catches from all licence holders. Clearly, adequate salmonid fishery conservation is greatly hampered by inaccurate returns of catches. As conservation to a large degree centres around re-stocking, it is obvious that the recommendation that no person be permitted to stock his waters without the consent of the River Board is pertinent: there is much more to stocking than the mere planting of fish.

Some valuable recommendations are made on the control by River Boards of the obstructions to migratory fish, and of particular importance in this context is the suggestion that a River Board should have the power to prevent any diversion of water unless sufficient water is passed down the original channel to avoid damage to fisheries. Equally sound is the point made that old dams, sluices, or leats in poor condition should be repaired by the owners, or alternatively demolished. Overcrowding in pools

* Report of the Committee on Salmon and Freshwater Fisheries. (Cmnd. 1350.) Pp. viii + 151 + 13 plates. (London: H.M.S.O., 1961.) 8s. net.

below unsurmountable weirs often results in dead fish during periods of high temperature and low water.

Pollution is dealt with admirably; and the suggestion that the Armer Committee's recommendations relating to the control of existing discharges should be extended to estuarine waters, should, if adopted, be highly beneficial to salmonid fisheries. I would like to see this control extended to all inshore waters.

The report stresses the need for more freshwater fishery research, and I am certain that the far too few freshwater-fishery biologists that are active in Britain will applaud the proposals that electric fishing and poisoning should be permitted for the purpose of scientific fishery research.

A report of this nature, which is of great interest not only to the 2½ million anglers but also to many industrialists, water undertakings and public bodies in Britain, will inevitably suffer much criticism. To a fishery biologist it suggests a beneficent revolution, but I find a few minor points of disagreement with the Committee. First, because only 2,000 netsmen are dependent, or partly dependent, on salmon fishing for their livelihood, because salmon anglers outnumber netsmen by about fifty to one and because anglers are of greater economic importance to River Boards, riparian owners, the tackle and tourist industry, I would recommend that netting be discontinued until salmon are much more plentiful. The netsmen should be adequately compensated. Secondly, I would recommend strongly that kelts should be preserved. As the report states, previously spawned fish form 5 per cent or more of salmon catches in the United Kingdom and thus represent at least 15 tons of fish; it would need several expensive hatcheries to produce this weight of fish. Thirdly, I would prohibit angling for salmon after the first week of September; in most cases this would protect the gravid fish and dispense to a large extent with the need for an accurate definition of unclean fish as discussed in the report. Fourthly, I would advocate a close season for coarse fish and that this be suited to local conditions. In addition, I would make it illegal for fishermen to return coarse fish above the minimum size, as many coarse fisheries are vastly overstocked and consequently the fishes are stunted. Lastly, I feel that the report should have stressed more strongly the need for research on coarse fish and I would suggest that this should be carried out in a national laboratory. I am certain that the River Boards and the River Boards Association would find such an institution invaluable.

Despite these objections the report is of major importance. It rightly emphasizes the remarkable achievements of the River Boards and its suggestions for the extension of their powers or for extended use of existing powers are expert, moderate and reasonable, as are its recommendations for compensation or non-compensation of affected interests. Everyone who has at heart the interests not only of the fisheries but also of rivers and lakes and the country as a whole will watch impending legislation closely and pray that the recommendations of the report become law with a minimum of mutilation and, above all, a minimum of delay.

J. W. JONES

ELECTRIC CURRENTS ASSOCIATED WITH GEOMAGNETIC DISTURBANCES IN THE AURORAL ZONE

By DR. F. J. SCRASE

Meteorological Office, London

THE electric current systems in the high atmosphere in high latitudes, especially along the auroral zones, are complex and variable. The normal networks of geomagnetic observatories suffice only to provide inferences of the broader features of these systems. With the view of their more detailed study, the Special Committee of the International Geophysical Year, at its Rome meeting in 1954, recommended that some observatories in auroral regions should be supplemented by two subsidiary stations, in directions at right angles and about 20 km. from the main station, for the measurement of the horizontal space gradients of the magnetic elements.

In an article amplifying these recommendations, S. Chapman[1] explained how knowledge of a disturbance in horizontal magnetic force produced by a horizontal current flowing directly overhead, together with the vertical gradient of the disturbance, will indicate the strength and height of the current separately. Since the divergence and curl of the magnetic field are both zero, the vertical gradients can be derived from the horizontal gradients.

In accordance with the recommendation of the Comité Spécial de l'Année Geophysique Internationale, two subsidiary stations equipped with Askania variographs were set up near the Meteorological Office Geophysical Observatory at Lerwick (60° 08′ N., 01° 11′ W.) and were operated for part of the International Geophysical Year and its successor the International Geophysical Co-operation, 1957–59. One of the stations was at Aipleton, 26·7 km. south-south-west of the main station and the other at Bridge of Walls, 22·3 km. west-north-west. This article describes preliminary results of attempts to estimate the magnitude and position of the electric currents during some magnetically disturbed periods.

Method of Analysis

The disturbances selected for measurement were mostly those of the bay type of a few hours duration, but a few were sharp peaks of shorter duration. For each station the components of the disturbance vector were derived by measuring the maximum deviation with respect to an undisturbed part of the record shortly before the disturbance, or to the interpolated undisturbed value at the time of the maximum. It soon became evident that values of the horizontal space gradients derived from differences between records from the pairs of stations did not conform with the theoretical relationships between

Table 1

Date		G.M.T.	*Rn* (km.)	*Re* (km.)	*Rz* (km.)	*C* $\left(\frac{\text{amp.}}{1{,}000}\right)$	*A* (deg.)	*I* (deg.)
1959	Oct. 26	15.25	+64	−3	53	+88	78	+11
1959	June 28	15.30	+105	−27	268	+368	91	−5
1957	June 4	16.40	0	−16	167	+86	95	−6
1959	June 27	17.05	+92	−67	371	+598	81	−9
1959	Oct. 3	17.05	+230	−230	627	+1,180	89	−19
1959	Apr. 27	17.10	+90	+11	156	+111	113	−9
1959	Sept. 1	17.20	+177	−64	248	+353	95	−18
1959	May 15	18.00	+50	−97	576	+750	91	−10
1959	Apr. 26	18.10	+109	+3	338	+235	99	−3
1959	May 11	18.20	+113	−60	352	+232	91	−10
1957	June 3	18.25	+87	−54	246	+465	83	−10
1957	June 21	19.45	+15	+34	66	+43	146	−4
1959	May 24	19.50	+173	+12	124	+157	90	+5
		Mean:	+100	−43	276	+359	95	−7
1959	Oct. 1	21.40	+34	0	48	−149	261	−6
1959	Apr. 26	22.07	+96	+15	74	−110	288	+12
1959	Apr. 27	22.31	+8	+25	55	−23	323	−7
1957	June 5	23.30	+53	+11	96	−203	294	+7
1959	June 4	23.43	+70	−39	77	−142	261	+2
1959	Aug. 22	00.30	+87	−14	224	−300	277	+6
1957	June 6	02.15	+36	−9	65	−98	279	+12
1957	June 18	02·30	+130	−39	82	−104	262	+14
1957	June 25	03.00	+70	−10	51	−75	248	−18
		Mean:	+65	−7	86	−134	277	+2

Key to headings of columns: *Rn*, *Re*, *Rz* are distances north, east and vertically from Lerwick of the intersection of the current with the perpendicular from Lerwick; *C* is the current strength, positive sign indicating west to east; *A* is the horizontal direction of the current with respect to geomagnetic north; *I* is the vertical inclination of the current, positive directed downwards.

the field components associated with a horizontal line current. Thus, the gradients of the north components along the easterly direction were found not to be equal to the east component gradients in the northerly direction. There may be several reasons for this. For example, the differences over finite distances, about 25 km., may not be representative of the true gradients at a point; or the assumption of a simple line current may not be valid; or the effects of induced currents or of local anomalies may be significant. A similar difficulty with regard to the gradients has been reported by Cain[2] in a preliminary analysis of differential measurements made during the International Geophysical Year in Alaska.

However, it is not necessary to know the gradients in order to compute the magnitude and location of the current that produces the measured field disturbances at two or more stations. The line current should, in fact, coincide with the intersection of the planes through the stations normal to the directions of the total disturbance forces. The location and direction of such a current, relative to the stations, are thus obtainable, by co-ordinate geometry, from the measurements at any pair of stations. With three stations three sets of results are obtained, thus affording some check on the accuracy of the determinations. Knowing the perpendicular distance R of the line current from a station, the strength C of the current is obtained from $C = f.R/2$, where f is the total disturbance force at the station. To avoid introducing a questionable factor, the effect of induced currents has not been taken into account in these computations, but the magnitude of the effect is considered later.

For the preliminary investigation, 22 records of disturbances were selected for measurement; these showed station differences in one component or another sufficiently marked to lead to reasonably consistent estimates of the currents. The disturbances (in total force) ranged from about 100 to 400γ, with station differences of a few tens of gamma. In some cases measurements from only one pair of stations were available.

The main results are presented in Table 1, in which the occasions are arranged in the order of the time of day at which the disturbance (maximum) occurred. No attempt was made to select disturbances according to the time of day, however, and their distribution may be taken to indicate that measurable disturbances occurred more rarely between 04 hr. and 14 hr.; this conforms with the almost complete absence of radar echoes from aurora in the morning daylight hours 06–12 hr. The fact that the results are confined to the period between April and October, however, does not indicate that disturbances rarely occurred in winter.

The computed locations of the currents relative to the positions of the stations are plotted in Fig. 1. In each case the relative magnitude and the azimuth of the current are indicated by the length and direction of the line, the mid-point of which represents the intersection with the perpendicular (3-dimensional) from Lerwick. It will be seen that most of the currents are located to the north-north-west of Lerwick between geomagnetic latitudes 62·5° and 63·5° N. Observations at the Balfour Stewart Auroral Laboratory from the area from the Western Atlantic to Central Europe indicated that auroral displays were reported on most of the days of the magnetic disturbances and that the bases of the auroral arcs were overhead in lat. 62°–67° and rays overhead in lat. 59°–64°.

Fig. 2 shows how the main features of the currents vary with the time of day. The most marked change is in the direction of the currents, which is from west to east between 15 hr. and 20 hr., and east to west from 22 hr. to 03 hr. This is in agreement with deductions made by Goldie[3] from magnetic storm disturbance vectors and also with the visual, photographic and radar echo observations of auroral move-

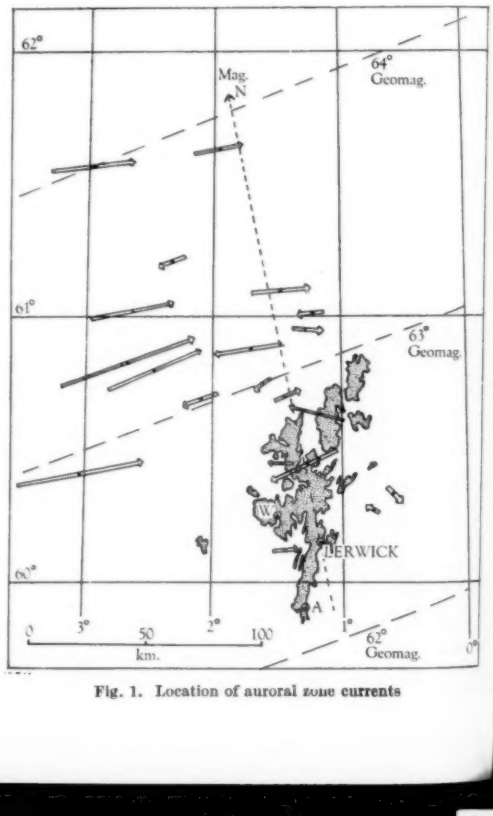

Fig. 1. Location of auroral zone currents

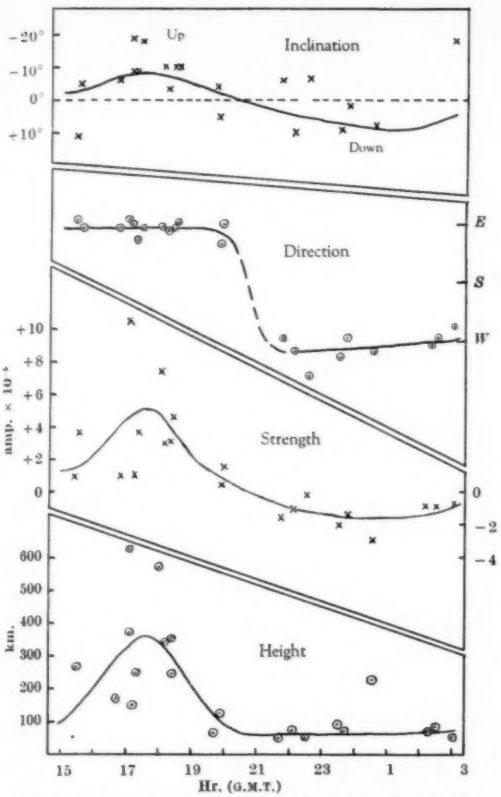

Fig. 2. Auroral zone currents derived from magnetic records

ments reported by various observers. The mean directions of the currents are more nearly parallel to the direction along local geomagnetic east and west than to the lines of geomagnetic latitude, the differences being about 5° and 15° respectively. This result is in conformity with observations of auroral rays (McInnes and Robertson[4]) and with recent auroral radio echo measurements (Watkins[5]).

The current strengths reach a maximum, of about 10^6 amp. in the west to east direction, between 17 hr. and 18 hr., fall to zero at about 21 hr. and then increase to a smaller maximum of about 3×10^5 amp. at about 0 hr. These current strengths are of the same order of magnitude as those determined by Goldie[3] for large magnetic storms.

The calculated values of the inclination of the currents to the horizontal (at Lerwick) show considerable scatter, but there does appear to be a tendency for the west–east currents to be inclined upwards and the east–west downwards by about 5°. This departure from the horizontal is too large to be accounted for by the Earth's curvature; at the extreme distance west of Lerwick of the currents shown in Fig. 1, the curvature should not cause an apparent departure of more than about 2°.

Probably the most interesting feature shown in Fig. 2 is the variation in the height of the currents. As in the case of the strengths, there is a maximum at about 17.30 hr. Between 15 hr. and 19 hr., most of the currents were at heights between 150 km. and 350 km., that is, at about the levels of the $F1$ and $F2$ layers of the ionosphere, but two appeared to be near 600 km. After 20 hr. and until 3 hr., the heights of the currents were centred at about 80 km., that is, near the E layer of the ionosphere and at about the average lower limit of auroral visual forms and of maximum auroral light. Most of the radio observations of aurora indicate that echoes originate from that layer, but radar reflexions have been detected from levels as high as 300 km. and observations of the scintillation of radio stars have indicated disturbances in ionization at these levels. From Goldie's determinations from magnetic storms of outstanding magnitude, heights of currents of between 230 km. and 390 km. at all hours of the day were deduced, but in storms of a more ordinary category the heights fell to about 100 km. between 0 hr. and 4 hr.

Effect of Induced Currents

It has already been mentioned that, in deriving the results described here, no account has been taken of the possible effects of induced currents in the Earth. That they are not negligible is shown by comparing the elevations of the vectors of disturbances recorded simultaneously at Lerwick and at Eskdalemuir about 500 km. south of Lerwick. Although the total disturbance forces recorded at Eskdalemuir are roughly of the magnitude expected at the distances from the currents computed from the Lerwick network observations, the elevations of the forces are all much greater than expected. This is also evident in the Eskdalemuir and Lerwick data used by Goldie[3] already mentioned; but fortunately he realized that the elevations at Eskdalemuir were more liable to be in error and used the total forces in his computations. The discrepancies in the elevations can, however, be largely accounted for if we assume the existence of an induced current, equal and opposite to the overhead current, at a depth of the order of 800 km. below the Earth's surface. Estimates of the depth in individual cases varied between 550 km. and 1,100 km. It is known that the electrical conductivity of the Earth, which is very small down to a depth of 600 km., where it has a value of 10^{-3} ohm.$^{-1}$ cm.$^{-1}$, rises rapidly to reach a value of at least 10^{-2} ohm^{-1} cm.$^{-1}$ at a depth of 900 km. (Lahiri and Price[6]).

In the method adopted for computing the overhead currents, the nearer the observing station to the seat of the currents the smaller are the errors caused by neglecting the induction current. If the effect of the latter at the depth of 800 km. is allowed for in the computations for the Lerwick network, the calculated heights and strengths of the overhead currents would be reduced by roughly 30 per cent, but the general trend of the daily variation shown in Fig. 2 would not be changed significantly.

Density of Ionization

If, as is now generally accepted, the currents of the auroral zone result from the movement of the ionization that is responsible for the aurora, it should be possible to relate the current strength C with the velocity of auroral motion V. At about 18 hr., when the currents reach their maximum value of about 10^6 amp. in the easterly direction, auroral speeds are of the order of 1 km. sec.$^{-1}$ in the westerly direction (Ratcliffe[7]). If, then, these currents are produced by the westerly drift of negative ionization at this speed and if the area of cross-section of the stream of ionization is A, then the electron density can be

estimated from $n = C/e.V.A$, where e is the electronic charge ($1{\cdot}6 \times 10^{-19}$ coulomb). Recent radar echo observations of aurora (Eastwood[8]) suggest that the region of ionization in which the reflexions take place is about 20 km. thick, and so an appropriate value of A would be 400 km.^{2}. These figures lead to an estimate of the electron density of:

$$n = 10^6/(1{\cdot}6 \times 10^{-19} \times 10^5 \times 400 \times 10^{10}) = 1{\cdot}5 \times 10^7 \text{ cm.}^{-3}$$

This is a reasonable value for disturbed conditions in the ionosphere.

This communication is published with the permission of the Director-General of the Meteorological Office. It is hoped to publish a more detailed account of the work in due course.

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GRAVITY SURVEY ON THE AIRA CALDERA, KYUSYU, JAPAN

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THE Aira caldera, in the southern part of Kyusyu, Japan, is one of the biggest in the world, measuring about 20 km. in diameter. It occupies the innermost part of Kagosima Bay, and an active volcano, Sakurazima (1,118 m. high), stands on its southern circumference. This volcano came into existence after the formation of the Aira caldera. The bottom of the caldera is the rather shallow (about 150 m. deep) and flat floor of Kagosima Bay and the steep cliffs occurring at several sea-shores have been supposed to be parts of the encircling caldera rim. A tremendous volume of volcanic ejecta, ignimbrite and pumice flows, which covers a wide area around Kagosima Bay, has been proved to have been ejected when the caldera was formed.

As reported before, my colleague and I[1] found a conspicuously low Bouguer anomaly on the Kuttyaro caldera lake in Hokkaido, which amounts to a maximum of 46 mgal. extending over 20 km. Since I considered such a low gravity anomaly as one of the characteristics common to the calderas of so-called Krakatoa type, the formation of which was accompanied by the ejection of vast quantities of matter, I tried to verify the similar gravity distribution on the Aira caldera. A gravity survey by means of a North-American gravimeter was carried out there in August 1960 in co-operation with the Geographical Survey Institute. Almost all the observation points are the bench-marks for precise levels. The Bouguer anomalies observed around the caldera and along the coast-line of the Sakurazima Peninsula are shown in Fig. 1, where topographical corrections are smaller than 1 mgal. for all observation points and can safely be ignored. As shown in Fig. 1, a relatively low anomaly is very conspicuous around the Aira caldera, the lines of equal anomaly being exactly concentric with the supposed caldera rim. At the northern boundary where there is no characteristic topography of the caldera rim, it is not easy to discriminate exactly only the effect of the Aira caldera on the gravity field, in other words, some residual anomalies due to the caldera, but one may easily extrapolate the tendency of decrease in the Bouguer anomaly observed on land to the centre of the caldera, though no observation in the Bay is available. At the north of Sakurazima, perhaps the minimum value of the Bouguer anomaly may reach about -25 mgal. and hence the relative anomaly to the surroundings may amount to about -45 mgal. Further, the centre of this anomaly coincides with one of the topographical depressions at the time of the great 1914 eruption of Sakurazima. Along the coast of Sakurazima itself, the anomalies are negative at the northern side and positive at the southern side, while the axis of inversion of the sign is almost the same as that of the crustal tilt which accompanied the recent activities of the volcano. The active volcano Sakurazima has scarcely any effects on the general distribution of the Bouguer anomaly on the caldera: this means that the size of the structure of a recent volcano cannot be compared with that of a gigantic caldera so far as density contrast is concerned. The anomaly suggested by the present survey, about

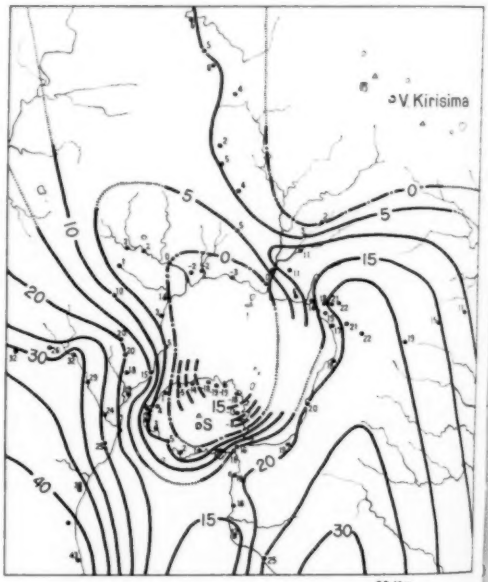

Fig. 1. Distribution of the Bouguer anomalies on the Aira caldera. Unit is the mgal. *S*, crater of Volcano Sakurazima

Fig. 2. Calderas in Japan

45 mgal. at its maximum extending over 20 km. in diameter, is very conspicuous and incidentally of just the same magnitude as the Kuttyaro caldera. Therefore, it is concluded that coarse material with a density about 0·3–0·5 gm./cm.³ smaller than that of its surroundings has accumulated at a depth of 3–4 km. beneath the caldera. The distribution of the Bouguer anomalies gives the total mass defect, which is responsible for the low anomalies, as $1{\cdot}6 \times 10^{11}$ metric tons. In fact, the material ejected from the caldera during its formation was deposited as ignimbrite and pumice flows around the caldera; its amount is estimated at $1{\cdot}6 \times 10^{11}$ m.³ by B. Koto[2].

Table 1

Caldera	Diam. (km.)	Age	Deposit	Bouguer anomaly (mgal.)	Observer
Kuttyaro	20–26	Pleisto-cene	Ignimbrite	−46	Refs. 1, 3
Akan	13–24	Pleisto-cene	Ignimbrite	−25	Ref. 3
Oosima	3–4·5	Holocene	Subsidence of basaltic lavas	+15	Ref. 4
Aso	17–25	Pliocene	Ignimbrite	−20	Ref. 5
Aira	23–24	Pleisto-cene	Ignimbrite and pumice flows	−45	
Volcano	**Height (m.)**	**Age**	**Rock**	**Bouguer anomaly**	**Observer**
Asama	2,542	Holocene	Andesite	Normal	Ref. 6
Huzi (Fuji)	3,776	Holocene	Basalt and andesite	Normal	Ref. 7

In Japan there are some ten calderas as shown in Fig. 2, some of which have been studied gravimetrically. The results of the observations on the calderas and volcanoes are summarized in Table 1.

From these data, though examples are not abundant, it can be seen that the Bouguer anomalies on the calderas can be classified in two types: (1) low anomaly, that is, Kuttyaro type; (2) high anomaly, that is, Oosima type. It can further be seen that they are almost normal on the large stratified volcanoes without caldera, such as Huzi (Fuji) and Asama, after consideration of topographical corrections. The two types of Bouguer anomalies correspond geologically to two types of calderas, the Krakatoa and the Glencoe types of H. Williams[8], according to the cause of their formations as discussed in ref. 1.

On the other hand, L. C. Pakiser, *et al.*[9] carried out a geophysical investigation in the Mono Basin, California, which is a volcano-tectonic depression surrounded by a large amount of pumice; they found a residual gravity relief of about − 50 mgal. extending over 25 km. It is very noticeable that the magnitude of this anomaly is of the same order as the Japanese large calderas. E. Medi and C. Morelli[10] made a gravity survey in Sicily and found no anomaly on Etna (3,274 m. high) as was also the case on Huzi and Asama. In effect, the contrasts of the Bouguer anomaly between large calderas and stratified volcanoes are very remarkable with respect to subterranean structure or depth of origin, shallow for the former and deep for the latter.

I wish to thank the authorities of the Geographical Survey Institute for their kind co-operation in this work.

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ETHYLENE EVOLUTION AND SUB-CELLULAR PARTICLES

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During the past ten years there have been at least five separate reports of the preparation of cell-free systems capable of evolving ethylene. The earlier claims relating to soluble systems, acetone powders and breis[1–3] have been criticized on many counts and in particular the specificity of several of the assay methods is questionable[4]. In fact, it is doubtful whether certain of the plant sources which were used actually produce measurable quantities of ethylene, for example, etiolated peas and oats[2]. More recently, the report by Spencer[5] of the preparation of a particulate ethylene-producing system from tomatoes has been disputed by Meigh *et al.*[6], who were unable to confirm the results using either tomatoes or apples. We also have had no success in preparing a sub-cellular system which evolves ethylene, using apples, tomatoes and the fungus *Penicillium digitatum* as sources of enzyme and the

techniques of Hall[1,2], Dimond and Waggoner[3], Spencer[5] and other conventional procedures. The most recent report of ethylene production by a particulate preparation is that of Lieberman and Craft[7], who used an acidic medium to isolate active fractions from both tomatoes and apples. It is the purpose of the present article to indicate that the volatile substance emanating from these particles is not ethylene, and hence that ethylene production by a sub-cellular system remains to be demonstrated unequivocally.

A single tomato in an early pink stage was homogenized at 4° C. with an equal volume of *p*H 6 buffer (0·05 *M* phosphate, 0·5 *M* sucrose), using a Waring blender for 1 min. at slow speed. The homogenate was passed through four layers of cheesecloth, after which 500*g* (15 min.) and 17,000*g* (30 min.) fractions were obtained by differential centrifugation. Each fraction was washed by re-sedimentation and the pellets were suspended in 5 ml. of *p*H 6·2 sucrose–phosphate buffer. Into a 5-ml. syringe were drawn successively 1 ml. of particles, the co-factors (listed below) contained in 0·6 ml. buffer, and 3 c.c. of air. The needles were sealed by insertion into rubber stoppers, syringes were incubated at 30° C., and finally the entire gaseous contents were analysed by pressing out all the fluid and gas chromatographing the remaining air on an aluminium oxide column. A flame ionization detector similar in type and sensitivity to that used in refs. 6 and 7 was used, so that the major difference between the present experimental technique and that described in their original communication in *Nature* is the substitution of an aluminium oxide adsorption column for the silicone oil partition column. However, it should be noted that details of the procedure by which 'acidic' particles[7] were prepared from tomatoes and apples have not yet been published, so that it is impossible to state with absolute surety that our sub-cellular preparation is comparable in all respects.

It was found that a gas similar to ethylene was evolved by both washed and unwashed 500*g* and 17,000*g* particles when adenosine triphosphate, co-enzyme A, magnesium chloride, phosphate and thiomalic acid were included in the incubation mixture (see Table 1: 'ethylene-like' substance). The rates per ml. are closely comparable with those reported by Lieberman and Craft, although an exact comparison is not possible since the nitrogen content

Table 1. Production of an 'Ethylene-like' Substance by Cytoplasmic Particles from Tomatoes in the Presence of Thiomalic and Thioglycolic Acids

Syringe contents*	μl./c.c. particles/hr.	
	'Ethylene-like' substance†	Ethylene
Particles + co-factors	0·0013	0·0003
Particles + co-factors + thiomalate	0·0343	0·0006
Particles + co-factors + thioglycolate	0·0314	0·0005
Particles + co-factors + glutathione	0·0038	0·0002
Particles + co-factors + cysteine	0·0041	0·0004
Boiled particles + co-factors + thiomalate	0·0023	0·0007
Particles + co-factors + thiomalate + EDTA	0·0035	0·0005

* The syringe contents consisted of the following: 50 μ*M* thiomalate, glutathione, cysteine or thioglycolate; 3 μ*M* adenosine triphosphate, 60 μ*M* phosphate; 0·08 μ*M* coenzyme A, 400 μ*M* sucrose, 25 μ*M* $MgCl_2$, 1·6 μ*M* EDTA. The *p*H of the reaction mixture was 6·2 and the syringes contained 1·6 ml. of liquid and 3 c.c. of air. The syringes were incubated at 30° C. for 60 min.

† These values are derived on the basis of a calibration of the flame detector for ethylene, but since the sensitivity of the instrument depends on the number of carbon atoms in the compound, the rates will be in error if the 'ethylene-like' substance does not possess two carbon atoms in its structure.

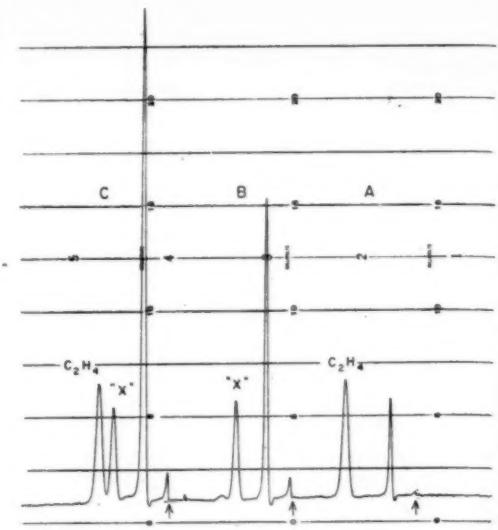

Fig. 1. Gas chromatogram of (*A*) an authentic sample of ethylene, (*B*) volatiles produced by tomato particles, and (*C*) a co-chromatograph of *A* and *B*. The graph reads from right to left and one vertical division corresponds to 30 sec. Samples were inserted at the times indicated with arrows. A 24-in. activated alumina column (Burrell) was operated at 23° C. with a nitrogen carrier gas flow-rate of 10 c.c./min. and a hydrogen flow-rate of 15 c.c./min. The first peak in each case represents a mixture of all unretarded components of the sample, and the 'ethylene-like' substance is labelled '*X*' on the graph

of our preparation was not determined. As shown in Table 1, the particles behaved in every respect in the manner which has previously been reported[7]: a requirement for thiomalate and thioglycolate is only very slightly replaced by cysteine and glutathione, the reaction is essentially inhibited by boiling the enzyme preparation, the evolution is strongly inhibited by 10^{-3} *M* ethylenediaminetetraacetic acid (EDTA), and both 500*g* and 17,000*g* fractions are active. In addition, the rate in the presence of thiomalic acid was found to be linear for almost 60 min. at 30° C. However, the gas which the particles produce does not co-chromatograph with added ethylene, as shown in Fig. 1, its peak being distinctly separated from that of authentic ethylene. Moreover, as shown by gas chromatography, neither bromine water nor mercuric perchlorate solution removed the substance from air, whereas these reagents were found consistently to eliminate comparable quantities of ethylene from synthetic air-ethylene mixtures. Thus it is established that the gas produced by these particles definitely is not ethylene. Since the substance is observed to chromatograph between ethane and ethylene, it may be a two-carbon compound, and it is most likely organic as it produces a response with the flame ionization detector.

A close examination of Fig. 1*B* reveals that there is a minute trace of a volatile material, amounting to a few per cent of the major unknown component, which has the same retention time as authentic ethylene. Its identity as ethylene was confirmed by the observation that treating the sample with either bromine water or mercuric perchlorate reagent caused it to be absent from a subsequent chromatograph. It is important to note that the production of ethylene, within the limits of accuracy which can be

obtained with such small quantities of material, is shown in Table 1 to be independent of the co-factors and substrates included, and the boiled preparation is at least as active as any other. Within a 25-min. incubation time, which was the briefest collection period investigated, the formation of ethylene had ceased in all cases. Therefore, the trace of ethylene is perhaps not due to a metabolic reaction, but rather to a contamination of unknown origin which is present, or formed by chemical reaction, at the start of the incubation.

Undoubtedly one of the stimuli which has prompted the search for a particulate preparation able to evolve ethylene is the observation that intact tissue responds to treatment with solutions of varying tonicity as though the ethylene-producing system were located in a particle having a semi-permeable membrane[8,9]. This evidence is indirect, and the interpretation can only be validated by the actual demonstration of ethylene evolution by a particulate fraction. However, the behaviour of mitochondria, which may display no endogenous uptake of oxygen although able to consume oxygen on the addition of appropriate substrates, makes it unlikely that particles will be shown to evolve ethylene unless a specific substrate is added. This situation is made even more difficult by the complication that cutting tissue into very small pieces seems to hinder ethylene production markedly[6,10].

In all properties investigated, the particles described in this article appear to be similar to the sub-cellular fractions prepared by Lieberman and Craft provided that our 'ethylene-like' substance is equated with their ethylene. If this is valid, then it must be concluded that none of the claims of ethylene production by cell-free systems can be supported, and that of Lieberman and Craft is no exception. It will be necessary to know more about the substrate from which ethylene is derived before such experiments are likely to be successful.

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OXYGEN ELECTRODE FOR MEASUREMENT OF TISSUE SLICE RESPIRATION

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SUBSTANTIAL progress has been made in using polarographic methods to measure oxygen concentrations in a number of biological systems, such as blood[1], mitochondria[2] and bacteria[3]. Improved reproducibility and simplicity of operation have been achieved by refinements in the oxygen electrode, for example, the Clark electrode[4]. The present article describes a continuously recording tissue slice respirometer which uses a Clark oxygen electrode covered with a $\frac{1}{2}$ mil 'Teflon' membrane. This system allows convenient measurement of rapid changes in respiratory-rates.

The tissue slice respirometer constructed in this laboratory is shown in Fig. 1. The reaction medium is kept uniformly mixed by magnetic stirring, so that addition of reagents does not alter the steady-state diffusion conditions for more than a few seconds. A sealed electrode vessel prevents oxygen transfer from the atmosphere. Thus, any oxygen consumed by the slice must come from the medium.

The uses of the tissue slice respirometer can be illustrated best by outlining the detailed procedure followed in a typical experiment. The results obtained in one such experiment are shown in Fig. 2. The first step was to fill the vessel with reaction medium and turn on the magnetic stirrer. The current-level corresponding to an air-saturated reaction medium (240 μM) was then measured. Next, the magnetic stirrer was turned off to reduce bubble formation to a minimum when introducing a slice of tissue. This accounts for the large decrease in electrode current evident in Fig. 2. A slice of heart ventricle from a rat was added to the vessel, which was then sealed with a vaccine bottle stopper containing an outlet syringe needle. About $\frac{1}{2}$ ml. of reaction medium was injected into the vessel to force any trapped air bubbles through the outlet syringe. Finally, the stirrer was turned on again, causing the electrode current to increase rapidly. When the new diffusion conditions reached a steady state (about 20 sec.), the electrode recorded the endogenous respiration (0·25 μmole oxygen/sec.), which is the slope of the curve at this

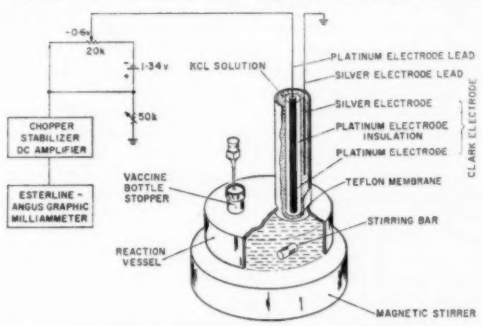

Fig. 1. Oxygen electrode system used for measurement of tissue slice respiration. The reaction medium is stirred rapidly enough so that the electrode current is independent of the rate of stirring. The electrode current is linearly dependent on the oxygen concentration (± 7 per cent) as determined by the measurement of the respiration of baker's yeast

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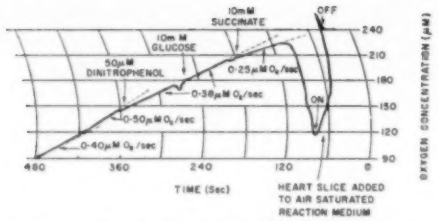

Fig. 2. Record of respiration of a slice of heart ventricle in several different respiratory states. Reaction medium containing 90 m*M* sodium chloride, 5 m*M* potassium chloride, 1 m*M* magnesium chloride and 40 m*M* phosphate buffer, *p*H 7·3. Temperature, 24° C. On–off refers to the magnetic stirrer

point. Injection of succinate into the vessel caused respiration to be stimulated by 50 per cent (to 0·38 μmole oxygen/sec.). Subsequent addition of glucose failed to stimulate respiration further. Following that, an injection of 2,4-dinitrophenol produced an additional 30 per cent increase in oxygen consumption (to 0·50 μmole oxygen/sec.).

This experiment illustrates how the oxygen electrode can be used to investigate the respiration of the same tissue slice in several different metabolic states. For example, the stimulation of respiration by dinitrophenol indicates that a low concentration of adenosine diphosphate in the tissue is limiting the rate of respiration in the presence of succinate. This agrees with recent findings of other workers, who used different methods for investigating muscle respiration[5,6]. The failure of glucose to stimulate respiration in the presence of succinate suggests that the penetration of glucose into the tissue slice is not rapid. If the glycose had penetrated into the slice, it would have been rapidly phosphorylated, thereby increasing the concentration of adenosine diphosphate. Any internally generated adenosine diphosphate would have stimulated respiration. No such respiratory stimulation was observed with the addition of glucose in the present experiment.

At the lower left corner of Fig. 2 the respiratory rate began to decline. This decreased respiration occurs at low oxygen pressures due to a decreased intracellular oxygen concentration. In practice, measurements are not made at such low oxygen pressures. By using an oxygen-saturated medium instead of an air-saturated medium, the respiratory rate of tissue slices is independent of the oxygen pressure over a much wider range of pressures.

It should be apparent from this that the oxygen electrode respirometer has many advantages over the manometric technique for work on tissue slices. The polarographic technique is especially useful for investigating the factors regulating respiration in tissue slices. Studies similar to that shown in Fig. 2 should facilitate greatly investigation of the action of chemical agents, such as drugs or hormones, on respiration. A relatively simple experiment can be used to determine whether such agents have any effect on the endogenous level of adenosine diphosphate, the substrate concentration, or the respiratory enzymes. Such information can be obtained by measuring respiratory rates in different metabolic states[6,7]. To obtain such information by manometric techniques would be much more time-consuming and less accurate. One can conclude from our work that a wider future application of the oxygen electrode for measurements of tissue slice respiration should prove extremely fruitful.

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PERSISTENCE IN ERYTHROCYTE GHOSTS OF MEDIATED SUGAR TRANSPORT

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THE special mechanism by which the common monosaccharides traverse the surface of the mammalian red blood cell has been the subject of considerable investigation in recent years[1–6]. Diverse experimental procedures, concerned primarily with analysis of the kinetics of sugar movements both into and out of the cells, have led to the conclusions that: (*a*) the permeation of the sugars through the cell surface occurs by way of a transient stoichiometric association with a limited number of reactive sites which are accessible at the cell surface to the two aqueous compartments (inside and outside the cell); (*b*) the translocation of the associated sugar-complex between the inner and outer interfaces of the surface layer results either from simple diffusion or from some kinetically indistinguishable process requiring no investment of metabolic energy. This mechanism, generally prominent in fœtal erythrocytes[2], does not persist at a comparable level of activity in the adult except in the human and certain other primate species. Typically, such cells are virtually impermeable to structurally related polyhydric alcohols and even to certain sugars, which, though isomeric to the transported kinds, are of unsuitable steric arrangement[4]. Moreover, application of a variety of blocking agents[1] may completely stop the penetration of the transported sugars without radically altering permeability to small unrelated solutes.

Thus the specific reactive sites are generally held to be located at the semi-permeable barrier classically identified with the plasma membrane, which would apparently restrain all monosaccharides from entering the cell, were it not for this special mechanism. This raises the question whether the red cell ghost, retaining many of the surface characteristics of the original cell would also display the specialized sugar-

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translocating behaviour which the barrier shows in the intact cell. It is known from Teorell's observations that ghosts may continue to take in glucose while still excluding sucrose[7]; but Teorell did not examine whether this could be attributed to a free diffusion or to a surviving mediated-transfer activity. This question was therefore specifically examined by quantitative comparison of red-cell suspensions and derived ghost suspensions in respect to D-glucose movements. These experiments indicate that the membrane transport continues to function at full capacity after removal of the bulk of the original intracellular material. Thus, densitometric records of ghost volume changes with time (Ørskov's method[8]) continue to follow satisfactorily the predicted relation:

$$kKt = (C_s + 1)(C_s + K)(C_s + K + 1)(V_0 - V) + (C_s + 1)(C_s + K)^2 \ln \frac{1 - V_0}{1 - V} \quad (1)$$

where k is a maximal flux constant, compounded from the diffusivity of the complex in the membrane and the fixed density of reactive sites; K is the dissociation constant of the complex and C_s is the external sugar concentration, both expressed in physiologically isosmotic units (isotones); and V_0 is the value at zero time of V, the intracellular (intra-ghost) water volume expressed as a fraction or multiple of its value at isotonicity. (This relation derives directly from integration of equation (1*b*) of ref. 6, for the case where the sugarless medium is isotonic and much larger in volume than the cells, as in Ørskov's procedure; though differently expressed, it is identical with equation (12) of ref. 3.)

Human blood was collected into standard ethylenediaminetetraacetic acid platelet packs ('Fenwal') from polycythæmic patients undergoing therapeutic phlebotomy, and stored at 2–4° C. when not used the same day. (In several years of related work on both normal and polycythæmic bloods, no indication has appeared of any relevance of this disease state to the sugar-transfer system.) Just prior to preparation of the ghosts, cells separated by centrifugation were taken through at least three successive washes in relatively large volumes of normal saline (Baxter) and finally two or three washings in a buffered balanced-salt medium[6]; in the course of this procedure, the major part of the white cells was discarded along with the uppermost red cell layers. Cells packed to about 75 vol. per cent in this medium were lysed by abrupt addition of (usually) 4 or 5 volumes of distilled water, and the hæmolysis reversed after 10–60 min. by addition of hypertonic sodium chloride, potassium chloride, or balanced salt mixture, sufficient to restore or slightly exceed isotonicity; finally, the ghosts were reconstituted by centrifugal washings in the isotonic medium and stabilized therein by 30–60 min. incubation at 37° C. before beginning experimentation. Cell and ghost volumes, counts and hæmoglobin contents were estimated by standard methods[6]; water contents by wet and dry weighings and sodium and potassium contents by flame photometry, through the courtesy of Dr. L. K. Dahl.

The ghosts prepared as described above were comparable with Ponder's preparations in retaining, typically, about a quarter of the original cell hæmoglobin and slightly less than half the original cell volume. In Ørskov's apparatus, they showed approximately the same transmission as intact cells at the same volume concentrations (that is, ghosts at about twice the cell count); this contrasts with the experience of Stein[10], who reported a marked increase in transmission at a given suspension concentration, with ghosts even less depleted than these. Densitometric comparison of these ghosts with intact cells in regard to glucose entry or exit at 37° C. showed essentially identical resistances to permeation: in response to a given applied sugar gradient, the ghost equilibration-rate was about double that of the cells, in appropriate proportion to the depletion of the original cell water and total solute. Compliance with this relation was further tested by varying widely the relative volumes of water used for lysis of several aliquots of ghosts, providing, after reconstitution, a series of ghosts of differing average volume. As shown in Table 1, the acceleration of equilibration progressed in pace with the depletion of the water space, implying an essentially unchanged membrane-transfer capacity even in the most shrunken ghosts.

Table 1. Acceleration of Glucose Equilibration with Progressive Depletion of Ghost Contents

Half-times estimated from Ørskov's recordings. Vols. refer to average single cell or ghost. $C_s = 0{\cdot}285$; glucose initially all in medium. k given on basis that $K = 0{\cdot}02$ isotone

Preparation	Total vol. (μ^3)	Water vol. (μ^3)	$t_{1/2}$ (min.)	k (isotones per min.)
Intact cells	95	68	0·86	1·51
Ghosts, from lysis at ratio				
1 : 2·5	62	45	0·65	2·0
1 : 4·2	43	32	0·41	3·2
1 : 8·3	33	25	0·30	4·3
1 : 16·7	24	19	0·23	5·7
1 : 33·3	17	13	0·18	7·2

This finding, that glucose was equilibrated in the ghosts at about the absolute rate expected from continued operation of the transport system of intact cells, made it highly unlikely that any other mode of transfer of sugar had come significantly into play. (Moreover, all these preparations retained the characteristic of intact cells of total impermeability to *m*-inositol, a cyclic hexahydric molecule physically similar to glucose.) The conclusion was further examined by study of the kinetics; Table 2 shows the similarity in the degree of slowing of glucose exit, in ghosts and cells, brought about by increasing the sugar concentration in the critical range. Had this equilibration occurred by way of uncomplicated diffusion, $t_{1/2}$ would be nearly invariant at all sugar concentrations, and K could be said to be very high (indefinitely greater than 10). But Table 2 shows the much lower K's at which the $t_{1/2}$ data give minimal dispersion in graphical fitting to equation (1); the K value for the ghosts is within the range generally reported for intact cells. It appears to be somewhat higher than that of the paired cell-suspension in the parallel experiment; but since these K's are approaching the lower limit for meaningful resolution by this technique and are subject to an uncertainty of perhaps one-third, no serious evaluation can, at present, be given to this possible minor shift in the affinity constant.

Table 2. Glucose Exit from Cells or Ghosts as Function of C_s

Prior equilibration $C_s = 0{\cdot}5$. Average ghost vol. = 46 per cent of average cell vol.

C_s (isotones)	Cells, $t_{1/2}$ (min.)	Ghosts, $t_{1/2}$ (min.)
0·009	0·24	0·10
0·022	0·36	0·16
0·034	0·59	0·20
0·051	0·74	
0·073	1·03	0·33
K (isotones)	0·014	0·024
k (isotones/min.)	1·8	3·5

The adequacy of these extensions of the indirect Ørskov technique was supported by direct chemical studies. Serial analyses of ghosts and cells in thick suspensions, during equilibration at 37° C. with glucose[6], gave a ghost k of 2·61 isotones/min. (again taking K as 0·02 isotone), while that for the intact cells was 1·65; in this instance the ghosts had sustained an average loss of 71 per cent of the original hæmoglobin, 46 per cent of the water, 49 per cent of the cell volume, and 95 per cent of the K^+, of which 25 per cent had been replaced by Na^+. The persistence of the specific transport activity after such radical alteration of the internal aqueous compartment argues strongly against the notion that the selective permeation is determined by the organization of a protoplasmic-water 'matrix', and points to the surface membrane as the locus of the specific reactive sites.

Membrane transports previously reported as surviving in the erythrocyte ghost under appropriate conditions include K^+ accumulation[11] and glycerol-mediated transport[10]. The former depends on supply at the time of lysis of a combination of metabolites serving later to energize the cation 'pump', so that it is difficult to establish a fair basis for comparing its capacity in the ghost with that in the intact cell. Glycerol transfer, however, is not thought to involve such complications, and would be expected to show the same reciprocal relation of equilibration-rate to residual ghost volume as noted here for glucose transfer. Stein's finding[10] of a rather small increase in $t_{1/2}$ for entry of glycerol into ghosts, as compared with cells, appears to run counter to this expectation; however, the residual volume of these ghosts was not specified.

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SEPARATION AND *IN VITRO* CULTURE OF CELLS FROM LIVER TISSUE

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A CELL-CULTURE examination was initiated to investigate more directly the role of the liver in the immunological response of rabbits to injection of antigen[1,2]. This involved the development of certain cell-culture techniques for the liver.

Most *in vitro* methods[3–9] for liver tissue have not involved separation of the reticulo-endothelial from the parenchymal cells although there have been a few such reports[10–12]. It is of distinct advantage to have separate cultures of the two types of cells, and in order to accomplish this, selection was made both in the initial isolation of cells and in the use of two different growth media. Sterility was observed throughout the following procedures, and solutions were warmed to 37° C. before use, unless indicated otherwise. A normal adult rabbit was bled from the heart, then killed by an injection of air. The abdominal area, which had been previously shaved, was swabbed with tincture of iodine, followed by ether. An incision was made and the liver removed. One lobe was perfused immediately (for parenchymal cell preparation), whereas the remaining tissue (for reticulo-endothelial cell preparation) was placed in 0·3 per cent trypsin prepared in GKN solution (8·0 gm. sodium chloride, 0·4 gm. potassium chloride and 1·0 gm. glucose in 1 litre distilled water).

Preparation and Study of Parenchymal Cells

The blood appeared to be entirely displaced by allowing 500 ml. of GKN solution to circulate through one separated lobe, the perfusion fluid being injected directly into the blood vessels through a hypodermic syringe needle.

As a final perfusion, the lobe of liver was injected with about 10 ml. of the culture medium ; this was 6 per cent 'Hyprotigen', an enzymatic hydrolysate of casein in aqueous solution (*p*H 6·7), containing 0·05 M sodium chloride plus 0·20 M potassium chloride. ('Hyprotigen' is the trade mark of Don

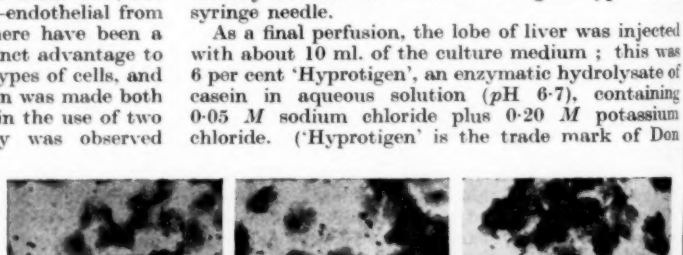

Fig. 1. Parenchymal cell culture from normal adult rabbit. *A*, 5 days after inoculation; *B* and *C*, both 10 days after inoculation. For all three fields, high-power object. × 43, oculars × 10, and photographic enlargement ~ × 10. Total magnification of *c.* 1,800 with emphasis of the fact that *A*, *B* and *C* are entirely comparable magnification

Baxter, Inc., Glendale, California; it is supplied also by some international licensees.) This lobe was allowed to incubate for 5–6 hr. in a beaker containing the same medium, placed in a 36–37° C. humidified incubator with a 5 per cent carbon dioxide in air atmosphere. At the end of the incubation the lobe was transferred to a large Petri dish (150 mm. diameter × 20 mm. height) containing about 40 ml. of the medium. For use in dispersing cells, stainless steel cloth (30 mesh, type 304) was fabricated into two rectangular pieces, each 10 × 4 cm.; three horizontal rows of wire were removed from one 4-cm. side and the other 4-cm. side was bent under as a 2-cm. flap for easier handling. The ends from which the wire was removed were placed into the tissue, one to grip the tissue and the other to be used as a rake to release the cells into the medium. A small area was 'raked' to give almost complete dispersion of the cells, then other adjacent areas of tissue were processed similarly until only connective tissue and blood vessels remained in an otherwise homogeneous suspension of cells. The former were discarded as debris and the cell suspension was placed in an Erlenmeyer flask from which equivalent aliquots were pipetted into 15-ml. conical, graduated centrifuge tubes.

These tubes were centrifuged for 4 min. at 650 r.p.m. (50*g*) in a clinical centrifuge with trunnion head. The supernatant was discarded and the pellet was used to prepare replicate cultures of 1/3 ml. packed cells per 5 ml. medium in Petri dishes of 60 mm. diameter × 15 mm. height. These dishes contained a glass coverslip. A zero-time smear of the inoculum was examined with Wright stain, whereas a May–Grünwald Giemsa stain was used for cultures obtained at various times after incubation in the humidified 5 per cent carbon dioxide in air atmosphere where temperature was regulated at 36–37° C. The stained smear showed that the inoculum consisted of parenchymal cells with no other cell type being obvious ; the majority of these cells were binucleated. After 3–5 days the cultures differed from those incubated for a shorter time both in number of cells and in the pattern of growth, that is, instead of a few isolated cells, there were fairly large groups of cells, and the cytoplasm was spreading, frequently as strands between the cells (Fig. 1, *A*). A determination of nitrogen in washed cells showed a slight increase after 5 days in culture, and at 10 days the nitrogen value had doubled compared with that for the inoculum. At 10 days there occurred amitotic activity in some areas of all cultures. The nuclear volume increased conspicuously, and the shape of the nuclei varied greatly (Fig. 1, *B* and *C*). These observations differ from those reported previously for liver tissue in culture but confirm quite precisely a discussion of amitotic activity as observed in human adult tissue just following autopsy[13].

Preparation and Study of Reticulo–Endothelial Cells

As soon as the perfused lobe was incubated, as noted above, the tissue which had been placed in trypsin was cut into pieces about 2–3 mm.[3]. With a pipette as much solution was removed as possible, and the tissue was transferred into an Erlenmeyer flask containing 100 ml. 0·01 per cent collagenase[14,15] prepared in GKN and cooled to 10° C. The suspension of tissue was stirred magnetically during refrigeration for 3 hr. The supernatant was poured from the tissue and 50 ml. trypsin was added. The pieces of tissue, suspended in trypsin, were stirred for 20 min. with an air-driven magnetic stirrer in the 36–37° C. incubator. It was very apparent that the enzyme treatment had reduced the tissue to a turbid suspension and connective tissue. The connective tissue was removed and the suspension of cells was centrifuged at 1,650 r.p.m. (330*g*) for 4 min. The supernatant was discarded and the pellet was washed twice in serumless medium (Table 1) for 4-min. periods at 1,650 r.p.m. (330*g*). About 1/3 ml. of pellet was inoculated per 5 ml. medium (Table 1)

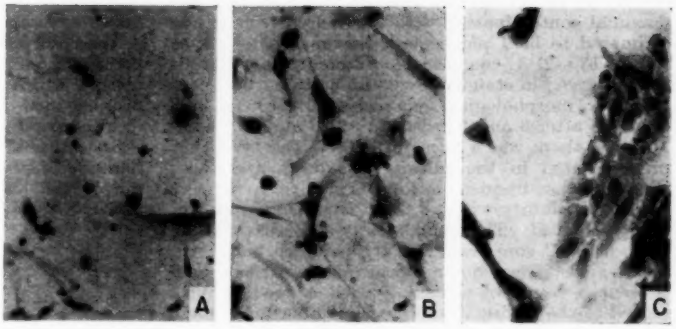

Fig. 2. Reticulo–endothelial cell culture from normal adult rabbit liver. *A*, 1 day; *B*, 3 days; *C*, 5 days after inoculation. Comparable total magnification of *c*, 1,800 for all three fields

Table 1. Tissue Culture Medium for Reticulo–Endothelial Cells

The quantities of amino-acids were derived as relative ratios from an analysis of the composition of rabbit antibodies (ref. 16), the remaining formulation developed from various experiments with splenic cultures, which do not differ significantly from reticulo–endothelial liver cell cultures

Amino-acids	Mgm./l. complete medium	Vitamins	Mgm./l. complete medium
l-Arginine (HCl)*	12·20	Choline chloride	4·0
l-Cysteine (HCl)*	8·14	Folic acid†	4·0
l-Cystine†	3·13	Nicotinamid	4·0
l-Histidine (HCl)*	3·79	Calcium pantothenate	4·0
*dl-Iso*leucine	17·56	Pyridoxal (HCl)*	4·0
l-Leucine	15·80	Thiamine (HCl)*	4·0
l-Lysine (HCl)*	16·15	Riboflavin	0·4
l-Methionine	2·80		
l-Phenylalanine	10·98		
l-Threonine	26·44		
l-Tryptophan	5·40		Mgm./l. complete medium
l-Tyrosine‡	13·24	Salts and carbohydrate	
dl-Valine	40·88		
l-Thyroxine (sodium salt)	0·025	NaCl	6,400
		KCl	400
l-Glutamic acid	23·60	$NaH_2PO_4 \cdot H_2O$	125
dl-Alanine	22·72	$CaCl_2$	200
l-Aspartic acid	19·34	$MgSO_4 \cdot 7H_2O$	200
Glycine	11·44	$Fe(NO_3)_3 \cdot 9H_2O$	0·1
l-Proline	17·12	NaHCO₃§	3,700
l-Serine	21·84		
Inositol	7·00	Dextrose	4,500

* Crystallized in hydrochloric acid.
† Dissolved in 1 ml. 1 *N* sodium hydroxide.
‡ Dissolved in boiling water.
§ Dissolved separately, then added to other salts.

The complete medium is prepared by combining in a 1-litre volumetric flask the solutions which are prepared separately, namely, the amino-acids in about 200 ml., vitamins in about 100 ml., salts and dextrose in about 500 ml. Then 1·5 ml. 1 per cent phenol red is added. The volume is diluted to 1 litre, flushed with carbon dioxide until colour becomes orange. The solution is sterilized in 'O_2 Selas', dispensed in aliquots and refrigerated for use over a period of 2–3 months. Just prior to use in cultures, the following are added to the final concentration indicated:

Rabbit serum (optional)	150 ml./l. = 15 per cent
Insulin	0·5 mgm./l., that is, 11 units
l-Glutamine	292 mgm./l.
Penicillin	10^5 units/l.

which contained 15 per cent normal rabbit serum. The maximal contamination with parenchymal cells was estimated to be 1 per cent of the inoculum. After 1 day (Fig. 2, *A*) considerable differentiation of cells was observed in stained cover-slip cultures, and conspicuous morphological change and growth had occurred after 3 and 5 days (Fig. 2, *B* and *C*). After 10 days, there was a sheaf of cells of the type that predominated in the culture at 5 days, and these showed rather frequent mitotic figures.

Reproducibility with all the above techniques was easily achieved. The commercially available medium 'Hyprotigen', containing mostly peptides and some amino-acids, was chosen for growing parenchymal cells as a result of screening various media. With regard to the medium described in Table 1, although this was less satisfactory for parenchymal cells than was the 'Hyprotigen', the omission of serum and addition of salmine protamine sulphate improved somewhat the cultural characteristics. It should be emphasized that a parenchymal cell culture does not require serum and, in fact, is inhibited by the presence of unmodified serum. Whereas peptides and amino-acids suffice for growth of parenchymal cells, the reticulo-endothelial cell culture does have an apparent requirement for serum. The rather general failure in the past with cultures of liver tissue may be largely attributed to the fact that it is extremely difficult to attain optimal conditions for a mixed cell population, and the more slowly growing parenchymal cell is rather rapidly suppressed by the reticulo-endothelial cell which differentiates and assumes fibroblastic characteristics.

These investigations are being continued with particular emphasis given to the parenchymal cell culture which slowly metabolizes, with secretion of protein into the medium, while demonstrating a characteristic amitotic activity.

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PHOTOPERIODIC EFFECTS ON STOMATAL BEHAVIOUR IN *XANTHIUM PENNSYLVANICUM*

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DIFFERENCES in stomatal behaviour under different photoperiodic treatments were reported by Schwabe[1] for *Chrysanthemum* and *Kalanchoe blossfeldiana*. He found that with short days (8 hr. daylight + 16 hr. darkness) opening of the stomata occurred towards the end of the night, but with long days (8 hr. daylight + 8½ hr. low light + 7½ hr. darkness) no night opening occurred.

We have investigated the relationship between photoperiodic treatment and stomatal behaviour using the short-day plant *Xanthium pennsylvanicum*. In some preliminary experiments effects similar to those reported by Schwabe were found: under short-day conditions (16 hr. nights) the stomata were closed for the first 11–12 hr. in the dark, but then began to open slightly. This opening was relatively small compared with that in high-intensity light (20,000 lux) during the day. In long-day conditions, in which the 16-hr. night was replaced by 8 hr. of low-intensity illumination (1,500 lux) followed by 8 hr. darkness, night opening did not occur. Light of 1,500 lux given after high-intensity light was not sufficient to maintain any appreciable opening, and consequently, under the conditions of these experiments, the stomata were closed for a larger proportion of the 24-hr. cycle in long days than they were in short days.

Considerable differences were observed in the rate of stomatal opening in light in the morning under different photoperiodic treatments. A detailed investigation has revealed a close relationship between the rate of opening in light and the length of the preceding night. In Fig. 1 it is shown that stomatal opening was much more rapid after 9-hr. than 7-hr. nights. The day-time illumination was 20,000 lux; the 9-hr. and 7-hr. treatments were, respectively: (*a*) 7 hr. of low light (1,500 lux) at the end of the day, followed by 9 hr. of darkness; (*b*) 9 hr. of 1,500 lux at the end of the day, followed by 7 hr. of darkness. It is interesting that 8½–9 hr. is the critical long night for flowering in *X. pennsylvanicum*. In a factorial experiment designed to study the interaction of length of night and the intensity of the light used to shorten the night, similar results to the above were obtained using either 160 or 40 lux, and even 10 lux had a significant effect in reducing subsequent rate of stomatal opening if given for 15 hr. followed by 1 hr. darkness. In another experiment, rate of opening after nights ranging in length from 1 to 23 hr. was examined; the different lengths of night were achieved by providing different periods of light of 1,500 lux at the end of the day, shortening a 24-hr. dark period. An almost complete cycle of 'opening ability' was found (Fig. 2).

In conjunction with these measurements, observations were made on the occurrence of night opening of the stomata. During a long period of darkness the stomata were first of all closed completely, but after

several hours they opened slightly, remained open for about 12 hr., and then closed again. The most important factor controlling the timing of this cycle of events was the time of termination of the low-intensity light. This is illustrated in Fig. 3, in which are recorded the times of the beginning of night opening under different illumination treatments. The plants had received high-intensity illumination (20,000 lux) until 4 p.m., and then various periods of low light terminating at the times shown. Prolonging the illumination delayed the occurrence of night opening, as indicated by the significant regression (continuous line). The broken line is the relation that would be expected if prolonging the low light by a given period had displaced the time of opening by the same period. The observed relation was significantly different from this ($P < 0{\cdot}001$), showing that the time from the termination of light to the commencement of opening was somewhat shorter after a longer period of low light. This indicated that the dark processes could go on in light of 1,500 lux, though at a reduced rate. The foregoing experiments were carried out at 27° C. In an experiment on the interaction of light and temperature it was found that in darkness night opening began sooner the lower the temperature in the range 15°–36° C.; in light of 10 lux it did not occur at all within 20 hr. of the change from 20,000 lux at 36° or 29° C., was delayed 1½ hr. at 22° C. and began at the same time as in darkness (after 9½ hr.) at 15° C.

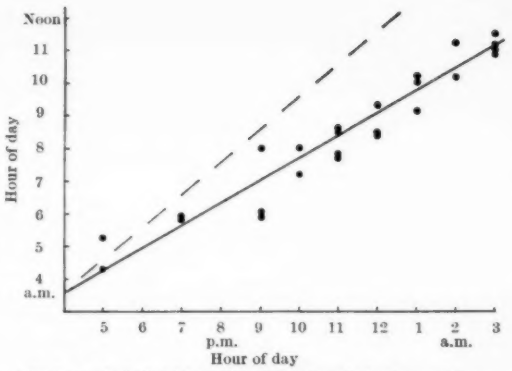

Fig. 3. Control of night opening of the stomata in *X. pennsylvanicum* by the time of termination of illumination. Ordinate, time of first detectable night opening. Abscissa, time of termination of light of 1,500 lux

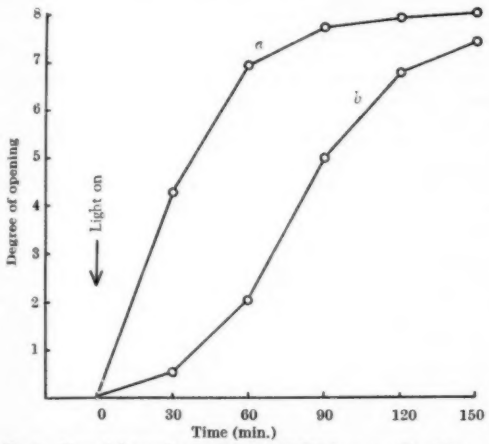

Fig. 1. Stomatal opening in light of 20,000 lux: *a*, after 9-hr. night; *b*, after 7-hr. night. Each curve is the mean from observations on six separate leaves. Ordinate scale, arbitrary units of total pressure minus pressure across the leaf, in a resistance porometer, that is, increasing values indicate opening

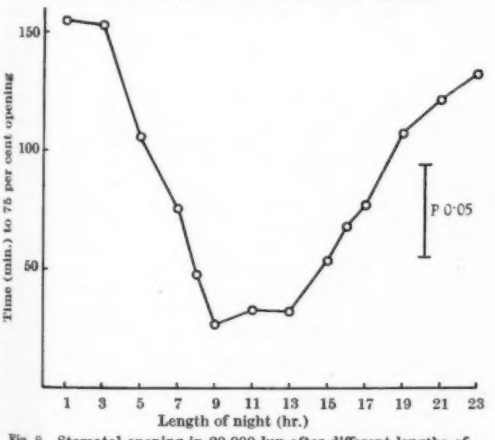

Fig. 2. Stomatal opening in 20,000 lux after different lengths of night. The time (min.) from the start of illumination to the attainment of 75 per cent of full aperture is shown. Each value is the mean of six observations on separate leaves. The least significant difference for comparison of two means at P 0·05 is indicated

We have been able to conclude that the differences in rate of opening in light after different lengths of night are related to the tendency for the stomata to open slightly for a time in continuous darkness. When they are already in the night-opening condition, however slight this opening may be, the rate of opening in light is very greatly increased.

The most satisfactory explanation of these observations would seem to be that an endogenous rhythm of 'opening ability' occurs in the stomatal cells in the dark, and that the phase of this rhythm is set mainly by the time of transition from light to dark, in much the same way as it was, for example, in the rhythm of carbon dioxide output in *Bryophyllum fedtschenkoi* reported by Wilkins[2]. We have not yet, however, shown that there is a true rhythm in which the cycle is repeated, though there has been some evidence of this in other species[3,4].

A full description of these and other findings will be published in due course, and an attempt will be made to relate them to theories of the stomatal mechanism. The results suggest that dark fixation of carbon dioxide might be concerned; this is known to occur in *X. pennsylvanicum*[5]. This aspect is being investigated.

The data were collected for attached leaves on potted plants using a new recording resistance porometer which automatically detaches and replaces *seriatim* the lower portions of the porometer cups on four leaves; the leaf surfaces within the cup areas were thus freely exposed to ambient air of normal carbon dioxide content except during the actual readings. This apparatus will be described elsewhere.

This investigation was carried out during the tenure by one of us (T. A. M.) of an Agricultural Research Council postgraduate studentship.

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[2] Wilkins, M. B., *Nature*, **187**, 523 (1960).
[3] Dale, J. E., *Ann. Bot.*, N.S., **25**, 39 (1961).
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INFLUENCE OF ELECTRICAL FIELD ON PUPATION AND OVIPOSITION IN *NEPYTIA PHANTASMARIA* STKR. (LEPIDOPTERA: GEOMETRIDAE)

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IT is known that weather often can affect insect behaviour and development profoundly. Electrical factors associated with weather may be partly responsible for these biological reactions[1], but very little experimental work has been done to determine if an electrical field can influence insect behaviour and development. Avio and Tarozzi[2] studied *Drosophila* oviposition in an electrical field (125 V./m.) but found no significant effects. More recently, Levengood and Shinkle[3] reported higher yields of *Drosophila* progeny reared in an electrical field. They did not mention, however, whether there was an observable effect of the field on time of development. Edwards[4] has reported some observations on the behaviour of adult *Calliphora* and *Drosophila* in an electrical field, with *Drosophila* responding to establishment of the field by temporarily reducing its activity.

Electrical field effects on the development of Lepidoptera, and on the location of eggs deposited by insects, have not been reported previously. Accordingly, some tests were made to determine whether an electrical field could influence pupation-time and the number and location of eggs deposited by moths of the phantom hemlock looper, *Nepytia phantasmaria* Stkr. This moth has one generation a year, and the adults live for about five days in the laboratory.

Four wooden boxes, 22 × 25 × 34·2 cm., contained the insects. Each box was covered on one side with a plastic screen and on the opposite side with a sliding glass wall. The boxes were placed on end, so that the glass wall could be opened slightly at the bottom by sliding it upwards. Inside, on the bottom of each box, an aluminium 'field plate', 19 × 14 cm., was suspended horizontally by two glass jars 4·5 cm. high. Directly beneath the plate, a plastic tray, 16 × 7·5 cm., with walls 2 cm. high, was placed between the two jars, and its inner bottom surface was covered with aluminium foil. The foil was connected to an insulated wire running through the box wall to the earthed negative terminal of an 810-V. battery-pack. The field plates in boxes I and II were each attached to an additional insulated wire passing through the box wall. For box I, the distal end of the field-plate wire was connected directly to the positive terminal of the battery-pack, thus establishing an electrical field (180 V./cm.) between the tray and the plate. A time-switch which alternated 'on' and 'off' periods of 5-min. duration (so than an intermittent field was established) was connected to the box II field-plate wire, between the box and the battery-pack. The field plates in boxes III and IV were wired directly to the trays; hence, there was no field between the plate and tray in these boxes. Boxes I and II were placed on top of boxes III and IV, and the unit of four boxes was illuminated continuously by a 60-W. bulb placed 8 ft. from the screened sides, so that all had approximately the same distribution of light.

Penultimate-instar larvæ were collected from Douglas fir in an outdoor cage, and allowed to pupate in the laboratory. Because all the insects did not pupate at the same time, it was necessary to add pupæ to the experiment on four different dates. Equal numbers of 1- to 2-day-old pupæ were put into each tray on August 8, 10, 12 and 15, to give a total of 98 pupæ in each box. Emergence, which began on August 22, was recorded daily by removing carefully the tray from each box through the sliding glass wall and counting the empty pupal cases.

The central portion of the *S*-shaped emergence curve was treated as a straight line, and a line was calculated for each set of emergence data by the method of least squares. The equations for the lines are: Box I: $Y = 12{\cdot}41 + 10{\cdot}53X$ ($\bar{x} = 3$; $\bar{y} = 44$). Box II: $Y = 18{\cdot}17 + 10{\cdot}75X$ ($\bar{x} = 3$; $\bar{y} = 50{\cdot}42$). Boxes III and IV: $Y = 19{\cdot}34 + 11{\cdot}10X$ ($\bar{x} = 3$; $\bar{y} = 52{\cdot}64$). The slopes of the lines did not differ significantly. An analysis of variance showed the emergence curve for insects in box I (constant electrical field), to be significantly later than the curve for the combined data from the control boxes III and IV ($P < 0{\cdot}01$). The 50 per cent emergence point for box I was slightly less than one day later than the control value. The line for box II insects (interrupted field) although shifted slightly towards a later time, does not differ significantly from the control curve. Table 1 shows that there were more males than females in all boxes, with the greatest difference in box I. However, the lag in the emergence from box I is not a result of the presence of these extra males. In fact, since males emerge somewhat before females in *Nepytia*, their predominance in box I should shift the emergence curve forward, not backward, in time. In other words, the significant shift occurred, despite the presence of more males.

Later, eggs deposited by the adults on all parts of the box, tray, and field plate were counted in each box. These data, together with pupal mortality and the numbers of each sex, are shown in Table 1. The mortality, although very low, was greatest in box I. There were also somewhat fewer eggs per female laid in boxes I and II than in the control boxes. The most definite difference, however, appeared in a comparison of eggs laid on the under-surfaces of the field plates with those on the upper surfaces of the plates. Chi-squared tests showed a significantly smaller percentage of eggs laid on the under-surfaces in boxes I and II than in boxes III and IV ($P < 0{\cdot}01$), with box I differing most from the controls. Where there was no field (boxes III and IV) the majority of the field-plate eggs were deposited on the under-

(*Continued on page 993*)

BRITISH ASSOCIATION MEETING IN NORWICH

THE HUMANITY OF MAN*

By Sir WILFRID LE GROS CLARK, F.R.S.
President of the British Association

IN his presidential address to the British Association for the Advancement of Science at Bristol more than sixty years ago, Sir William Crookes remarked that while formerly the President unrolled to the meeting a panorama of the year's progress in physical and biological sciences, now he usually restricted himself to speculations connected with his own work, or dealt with questions which for the time were uppermost. If I have both these objectives in mind in my address it is not out of sheer presumption, but because my own studies have for many years been related to man as he was in the past and man as he now is, and these studies naturally compel considerations of his potentialities for dealing with problems of the immediate future. Just as many individuals rightly seek to take stock of their personal achievements and failures at the end of each year and aim to correct their defects by resolving to carry out certain plans for the New Year, so I think it is now most necessary, because of the dangerous uncertainties looming ahead, to take stock of the species *Homo sapiens* whose evolution has culminated in mankind to-day. I believe it is our duty as scientists to insist that this is a matter of quite extraordinary urgency and to make it apparent as far and wide as possible. For, let us not deceive ourselves, the frightening question is now beginning to present itself whether the civilization which mankind has slowly and laboriously built up over a period of many thousands of years can avoid disastrous dissolution as the result of uncontrollable (or, at any rate, uncontrolled) struggles for political power or economic superiority, and, indeed, whether the human species can avoid at least partial extinction by the misapplication of its own ingenuity. This is not to be taken as a melodramatic statement—it expresses a truth which is quite evident to anyone who cares to read the signs of the times.

Naturally, in the course of a short address I can only make a very brief reference to some few aspects of my subject by attempting a rapid review of how we came to be and how we are constituted in our physical and psychological adaptability to cope with the changing environment in which we find ourselves to-day. The first part of this task is the easier, for the chronicle of the prehistoric story of human evolution and its bridge across the 'protohistoric' into the historical record now provides a reasonably close-graded developmental sequence the interpretation of which offers little difficulty so far as its main features are concerned. When the British Association met at Norwich in 1868, the objective fossil evidence for human evolution was practically non-existent, and when, three years later, Darwin published his *Descent of Man*, he had to rely for his thesis of man's relationship to lower animals almost entirely on the indirect evidence of comparative anatomy, embryology, the persistent relics of vestigial structures in the human body, and so forth. To-day the situation is very different, following the discovery of the fossilized remains of early hominids showing just those transitional characters which Darwin had postulated for 'missing links' between the simian and the human levels of evolutionary development.

Some of the earliest known of these precursors of modern man were the small-brained creatures, technically called the Australopithecines, which inhabited South Africa and East Africa as far back as the geological period of the Early Pleistocene, that is to say, half a million to a million years ago (or, according to the latest information, perhaps of even greater antiquity). These have commonly been referred to as 'hominids' rather than 'humans' because, although they are certainly early representatives of the zoological family Hominidae (of which we ourselves comprise the terminal and only surviving species), we do not know whether they had developed all those mental attributes which together are usually associated in common parlance with the designation 'human'. We know a good deal about their anatomy from the large quantities of their skeletal remains which have been found in a reasonably good state of preservation. The evidence is complete that they were erect bipedal creatures with a hominid type of pelvis, they had a dentition quite similar to that of modern and extinct types of man except for the large size of the grinding teeth, and many features of their skull show a remarkable approximation to their human successors. But their brain was quite small—in fact, scarcely exceeding in size the brain of a gorilla.

The first discovery of these primitive hominids naturally gave rise to much controversy, mainly for the reason that some authorities had become imbued with the conception (at one time widely held) that the initial stages in the emergence of the line of hominid evolution, and its segregation from the ape line, must have been conditioned by a marked increase in the brain volume. We now know that this conception was wrong—the expansion of the brain came much later. The acquisition of a bipedal mode of progression, that is, standing, walking and running on two feet, was presumably related to climatic changes resulting in a deforestation of the areas inhabited by ancestral precursors of arboreal habitat. This, of course, is a somewhat speculative explanation, but it remains clear enough that the liberation of the arms and hands from their climbing functions was of quite fundamental importance in permitting the development of a manipulative skill which came to be expressed in the ability to use and fabricate implements of stone and other material. From the evidence of recent discoveries in Tanganyika it seems a reasonable inference that, as had already been suspected from excavations in South Africa, some of the Australopithecines, in spite of the small size of their brain, were indeed tool-makers, and they also hunted game for food. Now, all these discoveries have been tremendously exciting for students of human evolution not only because they provide the direct evidence for Darwin's thesis of man's descent but because they also offer some of the earliest indications of the habits of life and the mentality that characterized human ancestry.

* Presidential address delivered in Norwich on August 30 and published in the September issue of *The Advancement of Science*, 18, No. 74 (1961).

Man has been defined, in contrast with lower mammals, as the tool-making animal; if this definition is accepted, then the terms 'man' and 'human' can perhaps be applied to the small-brained Australopithecines. It may be argued that the definition is not altogether valid because, for example, tame chimpanzees, presented with the problem of reaching food beyond their grasp, have been observed on a rare occasion to construct a tool by fitting two sticks together so as to make an implement long enough to reach a banana. But, apart from the fact that we have no evidence that apes in the wild state, that is, without some general training, are capable of inventing tools, one of the essential differences from the specifically human type of tool-making is that the ape can only construct an implement for a purpose which is immediately presenting itself; human beings, on the other hand, can construct tools in preparation for future contingencies, and this implies a capacity for mental imagery and foresight far beyond the level of any sub-human creature. We do not know just how far the Early Pleistocene hominids were capable of planning ahead in this way, but many of their tools, though of crude construction, not only show a certain uniformity of pattern which indicates a collective application by different individuals of a common technique in their manufacture, but there is also evidence that they were capable of making tools of several kinds as well as shaping pieces of bone to form pointed implements, suggesting a capacity for modifying the design of their implements for different specific purposes. This implies that they had already developed what can be termed a 'cultural tradition', at any rate in incipient form. The other important inference from recent excavations is that they were not only food gatherers but also hunters of game, and it has been pointed out that, if we can assume that the dietary habits of the modern apes were also typical of our ape-like arboreal ancestors, this indicates a rather radical evolutionary change from predominantly vegetarian to carnivorous habits.

The geological evidence makes it clear that the Australopithecines occupied a terrain of the savannah or veldt type to which they could only adapt themselves by developing such habits in association with their erect bipedalism. Moreover, it has been argued that the adoption of a meat diet must have had far-reaching effects on their social organization. For one thing, the initiation of hunting habits stimulated the need for implements of some sort that could be used for the killing and cutting up of game, and it also demanded co-operation within organized groups of hunters who had no natural weapons of attack or defence such as the powerful sharp canines which many other representatives of the order Primates possess. As Dr. Oakley has observed, "bipedalism is initially disadvantageous biologically unless there is some compensating factor—in the case of the hominids this was the ability to use tools and wield them while moving". Clearly, the Early Pleistocene hominids must have been particularly vulnerable in the open—they could only have survived in the form of hunting communities equipped with weapons of some sort. It may also be inferred that their ability to co-operate in hunting depended on a capacity for mutual planning in order to take concerted action for the benefit of the community as a whole.

The temporal successors of the Australopithecines (no doubt with some overlap in time in different parts of the world) are represented by an extinct type of man known as *Pithecanthropus*, whose remains have been found mainly in the Far East and who lived during the early part of the Middle Pleistocene, two or three hundred thousand years ago. They were more advanced in anatomical structure, with limbs of quite modern type, and with a brain of larger size though on the average only about two-thirds the volume of our own to-day. But they evidently lived in well-organized and co-operative communities, showed considerable skill in fashioning stone implements, were capable hunters, and even knew the use of fire for domestic purposes. There is now some evidence that populations of the *Pithecanthropus* type may have extended from the Far East into the African continent and perhaps elsewhere, but they were soon followed by (or were transformed by evolutionary modification into) more progressive types of later Middle Pleistocene age which show a closer approach to *Homo sapiens*. Indeed, so similar were some of these later types to the modern types that such differences as there are can only be demonstrated by elaborate statistical methods. Lastly, by the beginning of the Late Pleistocene, many parts of the Old World were populated by men who, so far as can be inferred from their skeletal remains, were no different from mankind to-day. Estimates based on radioactive carbon techniques have shown that *Homo sapiens* of modern type was already living in the Old World about fifty thousand years ago, and indirect evidence makes it probable that the antiquity of our species extends back to a time-level of the order of a hundred thousand years. The Palæolithic populations of Aurignacian times, who occupied Western Europe about thirty thousand years ago, had already developed a social organization of considerable complexity and were also responsible for some fine examples of cave paintings and sculptures.

By about 7000 B.C. the Neolithic Age had blossomed forth in the Near East with the discovery of the domestication and breeding of animals for food and other purposes, and the cultivation of edible plants which provided an important element of their diet. The agricultural surplus thus made available permitted the growth of settled and relatively stable communities in which the essential factor of the co-operation of individuals for the benefit of the community as a whole became much more obtrusive. Metal artefacts came into use in the Eastern Mediterranean with the discovery of copper and bronze for tool-making about 3000 B.C., and the technique of smelting iron was invented in Asia Minor soon after about 1200 B.C. (though the regular use of this technique did not spread into Western Europe until 500 B.C.).

A brief narrative of the major features of any line of evolution, such as I have just attempted, has the serious disadvantage of gross over-simplification, and tends to give an illusory impression that the evolutionary changes have been the result of a steady progress in which the evolving populations took a more or less passive role. But it is important to recognize that human evolution has by no means been a history of uninterrupted, unidirectional progress. For example, there were long periods of cultural stagnation in the Early and Middle Pleistocene during which it seems evident that primitive man must have been compelled to concentrate all his energies simply on keeping himself and his family alive in day-to-day existence, contending against almost overwhelming difficulties in the face of a harsh and unfriendly environment, and equipped with only the crudest weapons for his defence and with the most limited means of providing himself with sus-

tenance and shelter. We have only to think of the extreme climatic vicissitudes of the Ice Age to realize that the very survival of Palæolithic man in Europe and elsewhere could scarcely at that time have been prognosticated with any assurance.

I have noted that probably the main factor which determined the initial segregation of the hominid line of evolution was the adoption of an erect bipedal posture in adaptation to an open terrain, and that the expansion of the brain which is so outstanding a character of *Homo sapiens* came later. It now appears from the fossil record that the hominid brain did not begin to enlarge significantly before the beginning of the Pleistocene; but from the Middle Pleistocene onwards it expanded at a most remarkable speed—greatly exceeding the rate of evolutionary change which has so far been recorded in any anatomical character in lower animals. Now, with very few exceptions a progressive enlargement of the brain has characterized all groups of mammals since their early ancestors appeared more than fifty million years ago, but in the Primates the expansion began earlier, proceeded more rapidly, and ultimately advanced much further. The expansion also contrasted in the particular regions of the brain which were predominantly affected. Let me emphasize this difference in the following way. The forebrain (comprising what is termed the cerebrum) includes certain basal elements technically called the hypothalamus, amygdala and so forth, to which we may conveniently (but rather loosely) refer as the 'basal brain', and a larger mass covered by a convoluted layer of grey matter which we may call—also rather loosely—the 'cortical brain'. These two components are functionally interconnected, but while experimental and clinical observations have established that the 'basal brain' presides over our internal economy in the sense that it is fundamentally concerned with the regulation of essential bodily processes such as digestive, cardio-vascular and general metabolic functions, hormonal secretions, reproductive rhythms, etc., and also contains neural mechanisms underlying emotional reactions and their associated drives, the 'cortical brain' is concerned rather with the more discriminative aspects of sensory experience and motor control. It is these discriminative faculties which enable us to respond to the changing circumstances of our environment in a manner which is the most advantageous not merely to us as individuals but also to the community to which we belong.

To a variable degree in different mammals the more primitive functions of the 'basal brain' can be modified by the 'cortical brain' on the basis of the information which the latter is continually receiving (or has received in the past). In lower mammals this information permits a more accurate regulation of the instinctive activities which to a large extent dominate their behaviour. But in the higher Primates, particularly in man, the 'cortical brain' has come to assume an increasing control of the 'basal brain'. The conception at one time current in psychology, that mental experience can be separated into the rather sharply contrasted categories termed 'cognitive' and 'affective', has long since been recognized as misleading, for intellectual and emotional factors are so closely interlocked in any form of behaviour that they cannot be dissociated—even arbitrarily. Intellectual processes are only capable of full expression if they are activated and energized by the driving power of emotional processes which come to be associated with feelings of æsthetic satisfaction. Undoubtedly, one of the outstanding distinctions of the human brain is the enhancement of the interconnexions between the 'basal brain' and the 'cortical brain', to the extent that emotional drives may be made much more subservient to the ideational processes which are based on cortical activity. The latter can now, if needs be, hold these drives in check, or exploit them by directing them into channels serving the co-operative requirements of human society. Of course, many of us do not use this machinery of the brain as efficiently as we might do; but the point is that, in purely neurological terms, the machinery does exist to be made use of.

The expansion of the brain in hominid evolution involved all the co-called association areas of the 'cortical brain', but the enlargement of the frontal lobes—which give *Homo sapiens* the high forehead that is so distinctive of our species—is perhaps the most striking feature. Although the functions of the frontal lobes are highly complex, latterly we have learnt a good deal about this region of the brain from laboratory and clinical studies. It may now be accepted that the frontal lobes do not comprise the essential basis for intelligence *per se*, for even after they have been extensively destroyed by injury the intelligence of the patient may in some cases show no appreciable deterioration so far as can be determined by the usual tests. On the other hand, they are certainly of importance for making full use of intellectual potentialities in normal circumstances, because it is largely through them that the nervous mechanisms of emotional and instinctive drives which reside in the 'basal brain' are, so to speak, harnessed to the higher conceptual functions of the cortex. The loss of these controlling influences of the frontal lobes is demonstrated very clearly by interruption of their connexions with the 'basal brain' following brain injuries or leucotomy operations. In many such cases the patient is no longer able to adapt his behaviour to the social needs and conventions to which he normally conforms. He becomes impulsive, irresponsible, inconsiderate in his attitude towards his fellows, and unable to sustain intellectual activities in planning far ahead. In other words, he tends to become anti-social in his behaviour.

Observations of this sort give a clue to the factors which initiated the remarkable development of the brain in *Homo sapiens*, for it now seems pretty certain that it was the development of the distinctively human type of social organization, depending in the first instance on the use of tools and weapons in co-operative activities for foraging and hunting for food and for the protection of family groups, which demanded an accelerated development of those parts of the brain whereby emotional and instinctive impulses can be more effectively subordinated to the good of the community as a whole. The progressive elaboration of culture sequences which followed must also have depended on the inventive capacity of gifted individuals whose imaginative ability derived from a greater complexity of the cerebral cortex, and it is easy to understand that such individuals would confer a high survival value on the particular community in which they happened to appear. The rapidity of the evolutionary expansion of the brain during the Pleistocene is an example of what has been termed 'explosive evolution', and it emphasizes the extraordinary severity of the selective pressure which determined the initial appearance on this Earth of our own species.

Another factor, doubtless related also to the requirements of a tool-making and organized hunting community, which conditioned the rapid expansion of

the hominid brain, must have been the development of the extrinsic type of symbolization which we call language. There are, of course, other means of communication between individuals besides language—such as facial expression, gestures, the utterance of sounds which signal emotions of fear, pleasure, aggression and so forth. So far as sounds are concerned, Dr. Schultz has remarked that "the simian Primates are by far the noisiest of all mammals" but "as soon as the early hominids had ventured into open spaces, had begun to use and even make tools and had co-operated in hunting, the total variety of all means of expression needed additions which could only come from sounds". It is indeed difficult to suppose that even the primitive and small-brained Australopithecines could have developed the inventiveness required for a tool-making culture or could have collaborated in planned hunting expeditions if they had no verbal means (however elementary) for communicating instructions and for the social transmission from one generation to another of acquired skills. At any rate, the ability to interchange ideas by a system of verbal symbolization must certainly have been a prerequisite for the elaboration of the more-advanced and complex cultures of the Middle and Late Pleistocene, and the progressive perfection of such a system also provided the essential basis for the further development of the co-operative element which is so dominant a feature of the social organization of human communities. For verbal symbolization permits individuals to interchange information not only about things which immediately present themselves to the senses, but also about events of the past and possible contingencies of the future; more than this, it enables one individual to obtain a closer indication of the innermost feelings of his fellows and thus identify himself much more intimately with them psychologically. Correlated with these developments is the germination and progressive florescence of self-awareness, that is, the recognition of the self as distinct from the environment and from other selves, which, in time, expresses itself in what may be termed reflective consciousness. It is this sense of separateness, of course, which finally leads us on to the seemingly endless problem of how far the apparent realities of the external world reflect ultimate realities. Such a degree of self-awareness is the very basis of the consciously directed co-operative and altruistic element in human behaviour which is one of the main components of what may be termed the humanity of man.

If I had the time at my disposal I would like to elaborate the thesis that the emergence of the biological species *Homo sapiens* has been the resultant of selective pressures directly dependent on the invention and development of cultural organizations of increasing complexity. Thus the initial evolution of the human type of brain has been correlated with modifications in the differential growth-rates of the cranial skeleton, the reduction of the jaws and teeth, the perfecting of the locomotor apparatus, and the ultimate refinement of the structure of the hand for manual dexterity, and so forth. Even more important is the correlated extension of the period of immaturity during which the growing child remains dependent on the adult members of the community, for this not only makes possible the full post-natal development of the large and complicated brain, it also provides the opportunity for educating the individual to adapt himself to the intricate inter-relationships of human society. It may be said, indeed, that if man (in the broadest sense) invented culture, it was culture which invented *Homo sapiens*. Such a conception is not new—it has been simmering in the minds of anthropologists over the past few years, but it now needs to be more fully recognized.

Well, here we are to-day, the modern representatives of *Homo sapiens*, and up to now, after several hundred thousand years of rigorous striving and struggle in the face of adversities which at times must have appeared all but insuperable, our evolutionary success in the biological sense is sufficiently demonstrated by the fact that almost 3,000,000,000 individual specimens of our species are now scattered over the greater part of the world living in widely different physical and social environments. This itself is expressive of one of the outstanding features of our species—a broad and plastic adaptability rather than a narrow adaptedness to one particular set of conditions. But our biological adaptability needs to be constantly supplemented by deliberate and conscious efforts to adapt ourselves to local circumstances as they arise. In the exploration and exploitation of territories of climatic extremes, for example, or in trying to cope with the artificial conditions which we are continually creating for ourselves in different fields of human activity, we seek to find ways of adapting ourselves to new local environments or (if that is impossible) of adapting the local environment to ourselves. So far as physical and biological environments are concerned, we have some reason to be satisfied with ourselves in that we have hitherto met with considerable success by making use of all the resources of the physical and biological sciences and of the technological developments which have been derived from them, by the development of more effective methods of pest control and protection against disease, and also by intensive studies of the physiological and psychological factors underlying the adaptational potentialities of the human body. In the past several years much progress has been made in laboratory and field investigations of problems of climatic adaptation in man and animals, and closely related to this is the development of a field of research which has come to be termed 'ergonomics', that is, the study in the widest context of man in relation to his working environment. I need scarcely emphasize the relevance of ergonomics to industry, for this is now widely recognized for its importance in the rational design of machinery and in the control of the immediate climatic environment, whether natural or artificial, in which tasks need to be carried out.

But while we can feel some assurance of ultimate success in such material problems (so long as adequate funds and facilities are made available for their investigation by scientific methods), the sociological environment to which mankind as a whole needs to adapt himself presents a much more formidable problem. I have expressed the view that consciously directed co-operativeness has been the major factor which determined the evolutionary origin of *Homo sapiens* as a newly emergent species and the gradual development of the peculiarly human form of integrated society, and in this connexion I am reminded of the words of John Stuart Mill 125 years ago that "there is not a more accurate test of the progress of civilization than the progress of the power of co-operation". In the past this element of co-operativeness has led to the progressive assimilation of smaller groups into larger groups of increasingly complex social structure—at first families into assemblages of families, these into wider communities based on close

kinship, into clans and tribes, and later into the still larger economic and political units represented by States, federations of States, and the nations of the world. Naturally, I do not suggest that this process of assimilation has been a matter of even, steady, advancement—like the analogous process of biological evolution it has been chequered with long intervals of stagnation and periodic regressions. But there has been demonstrably a broad historical trend in the direction of a progressive unification of mankind. In some cases this has come about by mutual agreement for the furtherance of common interests; but it must be admitted that it has not unusually been the result of conflict between opposing groups ending in the subsequent mergence of the vanquished into the social and economic structure of the victor.

In so far as this process of unification has been to the ultimate benefit of the community as a whole by achieving a higher level of co-operation which, for example, permits a more centralized organization of all the means of production, more efficient control over local fluctuations in food supply, more comprehensive planning of large-scale projects of irrigation and transport, more economical division of labour by specialists of various kinds and so forth, it can be regarded as a major factor determining sociological progress. But the basic difficulty with the larger groupings of human society, as it seems to me, is that, with increasing population pressure, the altruistic sentiments which in the course of a long evolutionary history have become ingrained in the mentality of the *individual* are too apt to be suppressed by the narrow interests of each particular *group*. The larger the group the less control has the private individual in directing his innate impulses along channels which are ethically satisfying to him personally, for his attitudes and opinions come to be more and more directed for him by all the subtleties of propaganda developed in a complex society. At any rate, we now have to face the seemingly paradoxical fact that the larger the units of co-operation in social communities the greater the tendency for their segregation into populations opposed to each other by conflicting economic and ideological interests. Thus the historical process of unification has tended to slow up and ultimately threatens to come to a halt. If it has sometimes been the outcome of conflict in the past this can no longer be envisaged as practicable in the future, for a major conflict involving the nations of the world to-day must lead to a degree of destruction that would render precarious the very existence of mankind. We seek to escape from this impasse and we ask ourselves whether it is even feasible to hope for a final unification of mankind as a whole. At the moment few would be optimistic enough to give a reassuring answer to this question, and no doubt many would regard it as too unreal and idealistic to be worth consideration.

Yet one thing seems clear—an essential prerequisite for any attempt at further unification for the good of mankind as a whole is the ability to give full expression to the impulse for co-operation which is so characteristic a feature of the humanity of man, and this can only be made possible by opportunities for free intercourse and the free interchange of ideas among the populations of the world.

In the promotion of such an ideal I believe that science has a vitally important part to play, for fundamental science (the pursuit of knowledge for its own sake), together with technological science (in its application of this knowledge for the benefit of mankind as a whole), may surely be regarded as one of the culminating expressions of that co-operativeness on which depended the genesis of the human form of society in the distant past. I do not mean to imply that science among all other intellectual disciplines has a complete prerogative in this respect, but I do suggest that it is in a particularly favourable position to effect the closest contacts between peoples who are otherwise divided by political and other interests. For one thing, scientists of all countries feel impelled to retain and develop harmonious relations simply because they realize that the very advancement of their subject depends on a thorough acquaintance with work going on elsewhere—the progress of science is essentially the result of co-operative human effort. They may not all speak the same tongue, but they can and do speak with a common idiom and use a common terminology, and communication of knowledge and ideas from one individual to another can be more precise and certain in scientific phraseology just because the latter aims to be more carefully defined than phraseology of colloquial language. One may say, further, that there is a universality in the logic of scientific method which permits an intimacy of understanding scarcely possible in fields of learning where there are fundamental conflicts of opinion derived from preconceived convictions and beliefs based on unverifiable evidence, and not amenable to scientific methods of inquiry by the rigorous test of repeated observation and experiment.

In so far as science by its very nature imposes on the scientist and the scientifically minded an ethical code of profound respect for objective truth, it is to be regarded as intrinsically a self-regulating process. For any statements or judgments based on current scientific research must be open to continuous scrutiny in the light of subsequent research, with the freely admitted possibility that they may from time to time need to be revised or rejected. Co-operativeness is an integral element in this process of self-regulation and thereby contributes to the promotion of a sense of tolerance and intellectual humility. Finally, I would assert that the quintessence of the scientific mentality is to be found in a willingness to communicate and discuss new advances in knowledge with completely open-minded freedom, uninhibited by any attempt at secrecy for partisan or sectional interests of any kind whatsoever; it cannot be too strongly emphasized that secretiveness is the very antithesis of the spirit of free inquiry implicit in the scientific method. From the purely practical point of view, also, secretiveness acts as a serious brake on the advancement of science because the latter can only proceed actively through the unrestricted interchange and interpenetration of ideas among those engaged on problems of common interest.

In the past, the free intercourse between scholars of different countries by travel or correspondence was first demonstrated in the early days of the Renaissance period, when it was a common custom for scientists to undertake arduous journeys from Britain to the great medieval universities of the Continent to acquaint themselves with current advances in scientific knowledge. Even during the Napoleonic Wars and the American Civil War, as Sir Gavin de Beer has recorded in his book *The Sciences were Never at War*, scientists were able to travel unmolested through enemy territories. Unhappily, the situation has become very different in our own time, but I think it may be said that scientists in general all over the world are still striving to maintain a sense of unity in

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the face of circumstances which often appear very frustrating. For many years, of course, they have been in the habit of arranging international congresses in a variety of subjects, with a membership freely open to all those competent to contribute papers or join in discussions, and anyone who has participated in such congresses will have experienced the real sense of fellowship between man and man which inevitably develops in the course of probing problems of common interest.

Limited international scientific associations date back to the middle of the last century, but at the instigation of our Royal Society an association of a fully international character, the International Association of Academies, met for the first time in 1900. This was followed after the First World War by the formation of an International Research Council which met in Brussels in 1919. But unfortunately it came to assume a political character because certain nations were deliberately excluded from membership, and even when they were ultimately admitted in 1926 the stigma of political bias still remained. Finally, the International Research Council was dissolved in 1931, being replaced by the International Council of Scientific Unions, which aimed to be a truly international federation with the separate Unions retaining their individual autonomy. At present there are 13 Unions, including all the major physical and biological sciences, as well as the Union of the History and Philosophy of Science. From time to time the Council has formed special committees or joint commissions—for example, a Special Committee for the International Geophysical Year, another on oceanic research, and another on antarctic research. In all its multifarious activities, the international scientific unions have contributed much to international understanding, and I suppose the International Geophysical Year of 1957–58 was perhaps one of the most outstanding examples of the intimate collaboration between scientists of many nations for the benefit of mankind in general; it illustrates particularly well the important point that associations which are *wholly* international are essential for achieving full international unity. International scientific associations which are exclusive, in the sense that they admit membership of some nations and not others, can only engender suspicion and disharmony. It has even been suggested that the very term 'international' is psychologically objectionable because it presupposes and perpetuates the idea of the existence of separate national units. Certainly, in so far as international organizations are taken to represent Governments on which they depend for financial support (however tenuous this link may be), an undesirable element of political bias tends to be introduced.

Undoubtedly, one of the most important developments in science during recent years is the multiplication of international congresses, colloquies, symposia and so forth, and I take this to be an expression of the urgent need felt by the scientifically minded to maintain and extend still further contacts in a world which threatens to become disrupted by the conflict of national interests. Even more significant, I think, is an evident tendency to replace the term 'international' by more comprehensive and all embracing titles. Thus we have the Food and Agriculture Organization, the World Meteorological Association, the World Federation for Mental Health, the World Federation for Neurology and so forth. But apart from such specific organizations, individual scientific societies are themselves tending to become more international in scope and more cosmopolitan in their membership. For example, the Ergonomics Research Society, because of the importance of its work in so many fields of economic importance, attracts membership from individual scientists all over the world. One more point I should like to make here is the desirability more and more of supplementing or replacing 'national' research institutions by 'regional' research institutions for the study of problems of common interest in different geographical areas—in this connexion I think of current organizations such as the Arid Zone Commission, the Pacific Science Association, centres of research on locust control, and so forth. It scarcely needs to be stressed that the immediate and obvious advantages which accrue from regional research in the relatively undeveloped countries are more likely than anything else to remove any possible feelings of suspicion or distrust towards the more developed countries from which assistance is received. If the membership of the staff of regional institutions were unconditionally open to competent scientists of any community the welfare of which depends on the results of its research projects, this would do much to engender an atmosphere of concord and to dissipate any tendency towards national rivalry which may exist. In short, we need to aim for international co-operation, and not international competition, in scientific research—the full freedom of the one, and not the restrictive reticence of the other.

The British Association can with some reason claim a good share in the promotion of international amity through the medium of science, for it has always provided an open forum for the free discussion of contemporary problems which confront mankind not only in Britain but also all over the world. Its membership is a completely open membership, its Council is represented during each year at meetings of various scientific bodies in countries abroad, and for many years it has received as its guests at the annual meeting distinguished scientists from overseas. More than this, by its lecture services, young people's programmes, international youth science conferences, area committees, 'science fairs' and so forth, it has made increasing and impressive contributions towards the development of more widespread scientific education. Incidentally, I think it needs to be recognized, also, that the most important objective of the annual meetings of the British Association lies not so much in the mere recording and recounting year by year of new scientific discoveries, but rather in making clear their probable impact on the day-to-day life of individual citizens and the communities to which they belong, and, more important still, in demonstrating and explaining the principles and methods of scientific inquiry by which these discoveries have been made, with the inferential corollary that the same principles and methods can also be applied, with a considerable assurance of success, to other outstanding problems of a similar nature. In more general terms, our task is to inculcate as widely as possible that spirit of freedom in scientific inquiry which gives full expression to the deep-rooted altruism which, as I have already tried to emphasize, is an essential attribute of the humanity of man and which, I believe, can do more than anything else to preserve and intensify the feeling of harmony among all peoples of the world. But we need to press ahead in our efforts with the utmost vigour, for the dangers which now threaten the unity of mankind are formidable indeed.

And time is getting very short.

SUMMARIES OF ADDRESSES OF PRESIDENTS OF SECTIONS

THE INTERIOR OF THE EARTH

THE subject of the presidential address to Section A (Mathematics and Physics), given by Dr. R. Stoneley, was "The Interior of the Earth". Dr. Stoneley emphasized that since the interior of the Earth is not accessible, except by boring into the outer layers, knowledge of the properties of the interior is obtainable only indirectly, and often by a long chain of inference. Until the quantitative formulation of the laws of science, man's ideas on this subject were mainly uncontrolled speculations.

Before the present century, the shape of the Earth was known from geodetic measurements to be approximately an oblate spheroid of ellipticity about 1/297, a value confirmed by the variation of gravity with latitude. Laboratory measures of the constant of gravitation, combined with the value of gravity, gave the mass of the Earth, or equivalently its mean density. From the observed period of the precession of the equinoxes (26,000 yr.) the knowledge of the ellipticity, following Clairaut's theory of the figure of the Earth, gave the moment of inertia about the polar axis—a value too small to be consistent with the idea that the Earth can be a uniform solid, and implying condensation towards the centre. Towards the end of the nineteenth century a model was proposed by Wiechert which proved to be the precursor of the distribution of density now generally accepted; he envisaged a metallic core, of about the density of iron, of radius about 0·78 of the Earth's radius, with a rocky shell (or 'mantle', as it is now called) surrounding it.

Some evidence concerning the elasticity of the Earth was forthcoming from the observed free period of the variation of latitude, which for a completely rigid Earth should be about 305 days, and in fact is about 428 days: the lengthening is attributable to the finite rigidity of the Earth, which calculation shows to be of the order of that of steel. The study of Earth-tides agrees with this result. This evidence was insufficient for the determination of the variation of elasticity with depth.

Great advances have been made in the present century by the study of the records of earthquakes made at stations all over the world, generally far removed from the epicentre (that is, the place below which the shock occurs). In records taken at angular distances up to 108° from the epicentre, the first pulse to arrive is one of compression or dilatation (called P) like a sound wave; this is followed later by a distortional wave called S, and these waves, P and S, travel through the body of the Earth from the focus of the shock to the station. By analysing the times of travel to various distances one can find both the track of the ray and the wave velocity at all depths reached. At distances greater than 108° the appearance of the record changes, and the interpretation is that there is a sudden change at a depth of about 2,900 km. from a rocky mantle to a fluid core, which refracts the P pulses and does not transmit the S pulses; the core thus behaves as a fluid. The change of properties is sharp, for waves reflected at the core are received by surface stations at suitable distances. The many reflected, refracted and transformed pulses found on the records corroborate these findings. When the density is known, the elastic constants can be determined from the velocities of P and S; the density distribution is found by successive approximation, combining earthquake data with the known moment of inertia of the Earth, and making the reasonable assumption that the density does not anywhere diminish with depth. There appears to be a dense 'inner core' of radius about one-fifth of that of the Earth.

The structure of the continents and the ocean floor can be diagnosed by a study of the 'surface elastic waves', which are confined mainly to the outer layers. Where there happen to be a number of stations within 1,000 km. of an epicentre some information is forthcoming about the layering of the continents. Most shocks occur within a few tens of kilometres of the surface, but some deep-focus shocks take place at depths down to about one-tenth of the Earth's radius.

The determination of the temperature gradient in borings determines, where the heat conductivity is known, the rate of outflow of heat, which is much the same in all localities. This heat may in part be primitive, but its estimation depends on the Earth's thermal history, which is at present very uncertain; most of the outflow can be ascribed to generation of heat by the radioactive disintegration of elements in the outer layers, notably uranium, thorium and potassium. The difference in the layering of the continents and the oceans, however, poses problems that are still to be solved.

Magnetic observations are regularly made at a number of stations all over the Earth. In addition to the general (or 'dipole') field, there are fluctuations such as the solar daily and the lunar semi-diurnal variations; further, there are the storm-time variations attributable to changes in the Sun's activity. The analysis of these variations by Chapman and others indicates a marked increase in the electrical conductivity at a depth of the order of 400 km., a depth at which both the density and the velocity of earthquake waves appear to undergo a marked increase with depth. The origin of the dipole field has been the subject of much research work; most suggested explanations have been proved inadequate. The idea that the Earth acts as a self-exciting dynamo has been shown by Bullard to be a possible solution of the problem, but the analysis is intricate, and full details are yet to be worked out. The observed secular changes in the direction of the magnetic field at any place have been ascribed by Bullard to a movement in the core like that of a small number of large vortices circulating in a general westward direction.

A striking corroboration of the present picture of the density and elasticity of the Earth has been achieved in the past year by applying a 'frequency analysis' to earthquake records, to gravimeter observations and to Earth-tide data for some days following the large Chile earthquake of May 22, 1960. The periods of a number of free vibrations of the Earth had been calculated numerically, and the frequency diagrams show sharp peaks agreeing closely with the predicted periods.

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CHEMISTRY AND THE SPECTROSCOPE

THE presidential address to Section B (Chemistry) by Prof. R. G. W. Norrish, opened with a survey of the origin of the spectroscope, which was invented in 1859 by Bunsen and Kirchhoff. They built an instrument destined to reveal the forms of the cosmos: the infinitely great and infinitely small. A vast range of frequencies of electromagnetic vibrations has been revealed, of great significance to the chemist, and to it has been added in recent years the discovery of micro-wave spectra and their application to the study of molecular structure by nuclear magnetic resonance and electron spin resonance.

The principles of photochemical reactions were first conceived some fifty years ago through the work of Warburg, Bodenstein, Henri and Weigert, and took final shape through the work of others who followed. Into the whole framework have been fitted innumerable photochemical and thermal reactions the postulated mechanisms of which, involving atoms, free radicals or excited species, are entirely derived from the circumstantial evidence afforded by spectroscopic observations and quantitative measurements involving photochemical kinetics, and end-products.

In 1946 an attempt was made in Cambridge to obtain direct spectroscopic evidence for the free radicals postulated as intermediates in photochemical reactions. This was made possible by the discovery by Prof. G. Porter and Prof. Norrish that very large measures of photolysis could be achieved by the application of a powerful light flash to suitable reactants. These early flashes, produced by discharging a bank of condensers through argon, were of 1–2 msec. duration and dissipated up to 10,000 Joules of energy, and it seemed obvious that if free radicals were produced in the reactions, they must momentarily have been at such very high concentrations as to invite efforts to photograph their absorption spectra. This was achieved in the first instance by Porter in 1949, who, using a second flash as a photographic source triggered mechanically at short intervals after the first by the method of Oldenburg, was able to demonstrate the dissociation of chlorine and to obtain the spectrum of the CS radical by the photolysis of carbon disulphide. This early success was followed by the development of the technique; shorter photolysis flashes of 100 μsec. duration and of 1,000–3,000 Joules were devised, and shorter intervals between the photolysis flash and spectroscopic flash were achieved by using electronic delay circuits capable of precision timing to a few micro-seconds.

This new and powerful method of flash photolysis and kinetic spectroscopy affords a means by which the intimate nature of photochemical and thermal reactions can be studied and which yields objective information about the free radicals and other transient intermediates taking part. These intermediates generally have very short lives, measured in millionths of a second; nevertheless, it has been possible to photograph their characteristic spectra at successive intervals of time and so to measure the rates of their growth and decay during chemical reaction. Sometimes they are produced in a highly energized state of thermal vibration, yet 'cold' as regards their translational and rotational motions, and the study of this has thrown light on propagation of chain reactions, and transfer of energy from vibrational to other thermal degrees of freedom.

It soon became apparent that the early results of flash photolysis in the gaseous systems were vitiated by a great rise of temperature, which resulted in thermal as well as photochemical dissociation. Thus under the adiabatic conditions resulting from lack of temperature control, thermal cracking or explosion as well as photolysis may be induced in suitable systems, and the method of kinetic spectroscopy provides a valuable means for the observation of the rapid elementary reactions of free radicals or atoms occurring in explosive media initiated photochemically.

It provides, therefore, a comprehensive method which can be applied to 'instantaneous' reactions in the gas phase both under adiabatic and isothermal conditions, and also to reactions in solution.

The main development of the adiabatic method lies in the study of explosive processes such as the reactions of hydrogen, hydrocarbons, ammonia, phosphine, hydrogen sulphide, aldehydes, etc., with oxygen. If no chromophoric group is present, as in the case with paraffins, a sensitizer such as nitrogen peroxide must be added in relatively small quantity. Its function is to absorb the radiation of the photoflash and so to raise the temperature and generate at the same time free atoms or radicals which can initiate the chain-reactions of explosion. The growth and decay of free radicals such as OH, CH, C_2, C_3, CN, NH, NH_2, PH, PO, HS, HCO, etc., during the explosive process, can readily be followed during the few milliseconds of reaction. Much new information about these processes becomes available. In addition, the specific effects of additives on explosive processes, and in particular the mechanism of antiknock in the internal combustion engine, can thus be studied.

The method of flash photolysis when used isothermally in the presence of a large amount of inert coolant gas has proved to be a valuable tool for the theoretical chemist who is interested in the spectra and structure of free radicals. These may be generated photolytically by the flash, and entities such as ClO, NH_2, CH_2, CH_3, HCO, PH_2 and PO are providing a very interesting field of study.

Another aspect of the isothermal process of fundamental and wide interest to the physical chemist is following the reactions of atoms, radicals and molecules. In particular, mention may be made of the photolytic reactions of O_3, ClO_2, NO_2, which generate oxygen atoms and provide a means by which the elementary reactions of oxygen atoms with such substances as CH_4, NH_3, H_2O, O_3, ClO_2, NO_2, may be followed. Reactions such as $O + O_3 = O_2 + O_2^*$ yield highly vibrating oxygen molecules in the ground-state, cold as regards rotation and translation. In like manner investigations made of the reactions of other atoms and species such as Cl, Br, H have led to a generalization which may be formulated in terms of the equation $A + BCD = AB^* + CD$. If an atom reacts with a polyatomic molecule, a major part of the exothermic energy of reaction (up to 90 per cent in some cases) appears unequilibrated as energy of vibration in the newly formed bond.

In the cases cited the vibrationally excited molecule is produced by chemical reaction. In addition, it has been possible to populate the vibrationally excited ground-state of nitric oxide without reaction, by excitation to an upper electronic state followed by fluorescence to the ground-state. It is possible for the first time to put the measurements on a completely quantitative basis and to determine the absolute efficiencies of collisions with various other molecules

in removing energy from the vibrational to the other degrees of freedom.

Recently, Callear has succeeded in populating the $^3P_0$ state of the mercury atom by flash irradiation, and to obtain its absorption spectrum. The rate of decay of the spectrum can be measured photometrically, and the relative efficiencies of colliding molecules in degrading the electronic energy of this metastable triplet state determined.

Many reactions in solution may be studied by flash photolysis. In all such cases the system remains strictly isothermal. Reference is made in particular to the important investigations by Porter and his colleagues of the triplet or diradical state of aromatic hydrocarbons and dyestuffs.

The very short ultra-violet rays of the Sun, which do not penetrate the atmosphere, bring about reactions in the upper atmosphere, giving rise to the protective ozone layer and involving atoms and free radicals of the utmost importance to the geochemist and meteorologist; much information can be obtained about these by the methods described.

It is now believed that the primitive atmosphere of the Earth was reducing, composed mainly of hydrogen, together with methane, hydrogen sulphide, water, carbon monoxide and ammonia. If the solar system had a uniform origin there must have been influences which changed the atmosphere of the Earth and that of Mars from reducing to oxidizing. The first is the progressive loss of hydrogen from the relatively weak gravitational field of the Earth. The second is the photolysis of water and the other compounds, leading to the formation of formaldehyde and oxides of sulphur, phosphorus and nitrogen, which on interaction, according to the fascinating views of Urey and Oparin, deposited the primordial organic scum on the Earth and provided the necessary medium for the evolution of life. Many of the reactions involved can be studied by flash photolysis. Others, such as the photolysis of water, methane and other hydrocarbons, lie at present outside its scope, and provide a challenge to extend this powerful method to shorter wave-lengths of the ultra-violet than are at present available. (Full references to the work cited are attached to the report of the full address published by the British Association.)

BLACK SHALE, OIL AND SULPHIDE ORE

PROF. K. C. DUNHAM chose as the subject of his presidential address to Section C (Geology) "Black Shale, Oil and Sulphide Ore."

Black mud is accumulating at the present time in the following environments: peat bogs and swamps; eutrophic lakes showing stable water stratification; estuaries and related marine lagoons (for example, the Dutch Waddens); deep marine basins separated from the open sea by narrow straits or sills (for example, the Black Sea, the Norwegian fjords). The essential conditions for the preservation of the carbon compounds which provide the black pigment appear to be stagnant bottom conditions, or rapid sedimentation, or a combination of both. Deficiency of free oxygen is characteristic, favouring the growth of those anaerobic bacteria which convert sulphur and sulphate into hydrogen sulphide. Iron sulphide is forming commonly in black muds.

Consideration of ancient black shales indicates that similar environments existed periodically right back to the earlier part of Pre-Cambrian times; and at certain stages, it appears that black shale conditions covered far larger areas than at present. Examples include the Devonian Chattanooga Shale and its correlatives in the Eastern and Southern States of America, and the Kupferschiefer—Kupfermergel—Marl Slate of the European Permian.

The most important sources of the organic material from which the black pigment is derived are the land plants, which yield humus; and the marine phytobenthos and phytoplankton, which yield sapropel. The carbon/nitrogen ratio gives a clue to the provenance; it is initially higher in humus, and remains higher in its derivatives. On incorporation in sediments, humus yields coal-like derivatives, apparently composed of large complex molecules of high molecular weight. Sapropelic derivatives may yield liquid hydrocarbons on distillation (as in oil shales), and it may be true that the kerogen of sapropelic shales is capable of conversion by natural processes into small, light molecules of hydrocarbon; but the case for black shale as a universal source rock of oil has not been proved.

On the average, black shales exhibit a marked enrichment in certain geochemically rare metals, notably arsenic, copper, nickel, lead, uranium, vanadium and zinc, compared with ordinary shale or indeed with any other sedimentary rock. With the exceptions of uranium and vanadium these metals are probably combined mainly as sulphides, the formation of which was aided by microbiological processes and a reducing environment. It must not, however, be supposed that all black shales are enriched in these metals; two examples from the Yorkshire Jurassic, analysed for this purpose, failed to show abnormal amounts. The determining factor is evidently the availability of the metals in the lake or sea water; if they are present in unusual quantity, black shale environments will favour their retention.

Some of the major sulphide ore deposits of the world occur as concordant beds or lenses in black shale. The Kupferschiefer of east-central Germany is a notable case where there is strong evidence for the contemporaneous deposition of copper, lead and zinc with shale. Though the bed is only thin (it averages 22 cm.), it is metalliferous over hundreds or even thousands of square kilometres. Other possible examples include the White Pine copper deposit (Michigan), the very extensive ore-bodies in the ore shale of the Rhodesian Copper Belt, and the lead–zinc ore-shoots of Mount Isa (Queensland). The total mass of such deposits is insignificantly small compared with the total mass of all black shale; and it is therefore suggested that very special conditions were required to produce them. An attractive hypothesis is the coincidence of springs of hot, metal-bearing water ejected from crystallizing igneous bodies at depth in the Earth's crust, with stagnant hollows in the sea-floor, where black mud was accumulating. There is, however, no actual support for such a hypothesis; what is needed is an investigation of the bottom muds of seas in regions of present igneous activity.

Concordant sulphide ore-bodies are most common in the older Pre-Cambrian rocks, in Canada, in Fennoscandia, in Australia and elsewhere. Here metamorphism has usually obscured some of their original characters; but where graphitic schist is associated, it is tempting to think that they may originally have been syngenetic bodies in black shale.

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LIFE AND ENVIRONMENT ON THE BED OF THE SEA

THIS was the subject of Prof. C. M. Yonge's presidential address to Section D (Zoology). In his opening remarks he reminded those present that the demersal invertebrates of shallow seas are divisible into epifaunal and infaunal animals, the former living largely on hard, and the latter within soft, substrata. While the first have diverse feeding habits they are less specialized than members of the burrowing infauna which are, however, restricted to suspension feeders, deposit feeders and carnivores. Abundant in all latitudes, the infauna is only about one-fourth as rich in species as the epifauna, which is most diverse in the tropics.

Initial populations were probably either epifaunal or pelagic and the establishment of soft substrates must long have preceded the evolution of organisms able to exploit their possibilities and add to these by formation of organic detritus. In shallow water the finer grades of bottom material came increasingly to represent food as well as shelter. But, being largely without oxygen and demanding elaborate burrows, their invasion was doubtless the final stage in colonization of the sea bed.

Sponges and cœlenterates are essentially epifaunal groups, the latter exploiting this habitat with supreme success culminating in the formation of coral reefs. Annelids, crustaceans and echinoderms are also largely epifaunal, although many have invaded soft substrates. The Mollusca were also primitively epifaunal and, at least in so far as they were ancestral to modern classes, more simply constructed than *Neopilina*.

Of these, the Gastropoda represent the major epifaunal, and the Bivalvia the major infaunal, class. The Archæogastropoda retain the basic habit but evolution of structure in the Mesogastropoda permits invasion of soft substrata, for example, the ciliary feeding *Trichotropis* living on bottoms largely of dead shells while *Turritella* and *Aporrhais*, respectively suspension and deposit feeders, burrow in mud. Carnivorous mesogastropods, notably naticoids, invade soft substrates feeding largely on bivalves. This is also true of some Opisthobranchia.

The Bivalvia are the supreme exponents of the infaunal habit. Lateral compression renders penetration easy, enclosure and then loss of the head by the mantle/shell makes them obligatory suspension or deposit feeders. Initially burrowing superficially with anterior inhalant current, movement of this posteriorly with development of siphons enabled bivalves to exploit all soft substrates. Deep burrowers evolved independently in four suspension-feeding groups as well as in the deposit-feeding Tellinacea. From the former came rock borers such as *Platyodon*.

Exploitation of the epifaunal habitat came by retention of the initially post-larval byssal apparatus. Such attachment of the body led to changes in its proportions, to heteromyarianism as in mussels (Mytilidae) and to monomyarianism as in scallops (Pectinidae). This could lead to cementation (attachment by mantle/shell) as in oysters (Ostraeidae) or to freedom as in many scallops, that is, to epifaunal life on soft as well as hard substrata. The infaunal habit may be regained by byssally attached heteromyarians either by boring into rock, mechanically (*Botula*) or chemically (*Lithophaga*), the shell becoming secondarily symmetrical, or by vertical attachment within soft substrates as in the Pinnidae.

TECHNOLOGY AND GEOGRAPHY

THE presidential address to Section E (Geography) by Prof. S. H. Beaver was entitled "Technology and Geography", and emphasized the fact that all human activity takes place in an environment which has three major aspects, physical, technical and economic (including social). Each of these is subject to change—though the first is the most stable—and each will vary in its importance according to the activity.

The physical environment comprises the underlying geology, the soil and vegetation cover, the surface configuration of the land, and the climate. It is virtually immutable by man, though to an increasing extent modern engineering techniques are making it possible, if not to remove mountains, in any event to divert rivers through their watersheds and to carve up the land surface with very striking effect.

The economic environment is of immense importance in almost all human activity, for very few parts of the world are completely without money or other tokens of exchange. All productive activity must be related to costs of production (including interest on borrowed capital, costs of raw materials, fuel and power, transport, labour) selling prices, tariffs and taxes. Here, clearly, is an ever-changing environment and one capable of much manipulation by man.

The technical environment comprises the technical aids which are available to assist in the process of land occupation and development of resources. Here is an environment of great complexity, from the primitive digging-stick and water-wheel to the blast furnace, the chemical insecticide and atomic energy; an environment which varies immensely from time to time and from place to place.

The interest which the technical environment has for the geographer is concerned largely with the changes in geographical values which are brought about by inventions and innovations. Within the total geographical environment all the many components have their respective values in relation to any particular activity at any given time. Alter one of the components and the relative values of others will change.

The examples given in the address—mainly from the field of mineral-working and mineral-using industries, but including also electric power and motor-car manufacture—seem to lead to the conclusion that rather than emancipating man from control by his environment, technical advancement often works the other way, narrowing appreciably the variety of locations in which the more advanced techniques may be practised. Thus the widespread distribution of the pre-Industrial Revolution iron industry was considerably reduced by the invention of smelting by coke, for only a few British coalfields possessed coking coals and iron-ores in the same strata; the invention of steel-making processes narrowed the range of possible localities still further, while with the development of the modern integrated works, increasingly dependent on foreign iron ores, only a mere handful of possible sites remains, as the post-war history of the industry well shows.

Old-fashioned thermal-electric generating stations were widespread, depending largely on the distribu-

tion of the urban population; but modern power stations are severely localized by the availability of the cheapest possible coal supplies (that is to say, from our lowest-cost coalfields), of enormous quantities of cooling water and of sites for the dumping of pulverized fuel ash. The introduction of hydro-electric generation encounters even more severe location problems, while nuclear stations also have but a very narrow range of sites to choose from. Another example from a different field is the oil-refining industry; the technical advances in ship construction which have rendered possible the 100,000-ton tanker have faced the oil industry with an immense problem in the location of new refineries, and the number of possible new sites is very small indeed.

It is not necessarily always so. The advancing techniques of mechanical excavation have considerably widened the range of open-cast mining in the ironstone fields of the east Midlands. The conversion of the motor-car industry from an essentially hand-engineering business to an automated assembly industry has to a large extent emancipated it from the former control (in the west Midlands) by a pool of highly skilled labour and a vast army of component industries, and has enabled it to be almost a foot-loose industry, attracted to sources of semi-skilled labour and port facilities in London, Merseyside, Glasgow and South Wales. These changes, however, if they do not support the thesis of intensified geographical control over location, at least support the contention that technical advancement materially alters pre-existing geographical values.

THE ECONOMIC USE OF BRAINS

WE are reminded by Prof. C. F. Carter in his presidential address to Section F (Economics) that Britain is not just short of scientists and technologists; it is short of high ability of almost every kind. This deficiency is likely to become increasingly important, and it will at many points limit the speed of economic and social change. Perhaps because 'ability' is a vague concept, relatively little attention has been given to the right use of this very scarce part of Britain's natural resources. Prof. Carter's address was a plea for a scientific approach to the matter and for much more research about it.

Many people have regarded the supply of ability as being like that of water (in England)—ample, provided it is conserved. The evidence for this is found in studies which show the strong social influences on education, and give rise to a presumption that much innate ability runs to waste if it is born in the wrong social class. The conclusion usually erected on this foundation is that, if we had more and better education, the supply of able people would be increased; but what the evidence in fact shows is that the supply would be increased if we had a different social system, which is quite another matter. It is possible, also, that the wastage of ability is greater in the middle and lower ranges than among those of really outstanding qualities. More research is needed before it will be safe to conclude that there is plenty of high ability available, at present running to waste, and easily conserved.

More research is also needed on the use made of able men in industrial administration, scientific work, the Civil Service and other occupations. Their work-load is inevitably, and to some extent desirably, a mixture of easy and difficult tasks, and it is not a practical rule to say that they should never be doing anything which could be done by someone of lower qualifications. No doubt the vagueness of the subject-matter will prevent any precise results being obtained from a study of what able men actually do with their time, but it seems likely that there are further principles to be discovered.

The distribution of ability between occupations is affected to some extent by mobility, but mainly by the initial choices of occupation made at various dates during the past half-century. We know that these choices are influenced considerably by decisions about specialization at school, and that these decisions are made with inadequate information, either about the potentialities of the boy or girl, or about the advantages of particular occupations to which the specialization will lead. We know that, in the initial choice of occupation, matters of fashion and social prestige play a considerable part. There can be little relation (except by accident) between changes in the distribution of ability and the national need. Indeed, in a community which has no strong sense of national objectives, it may be exceedingly difficult to divert lively and able minds from the occupations which are fashionable and relatively well supplied with ability to those which most need its help.

It would be interesting to know, also, whether there are economies of scale in the use of the various forms of ability. If there are, and if the innate qualities for high ability appear equally often in all nations, then those large countries the social systems of which do not frustrate ability will have an advantage over small nations, and (as Dr. B. K. Blount has suggested) China can in time be expected "to produce ten times as many scientists of world repute as we can".

WORLD COMMUNICATIONS

IN his presidential address to Section G (Engineering), Sir Gordon Radley directs attention to the fact that the past few years have seen the start of a rapid growth in the facilities for world communication using trans-oceanic telephone cables. The trans-Atlantic cable completed in 1956 was the first project of this kind. It was only made possible by the development of underwater electronic repeaters for the amplification of the speech signals at intervals along the cable. The project involved the installation of about 300 valves and 6,000 other components in pressurized steel housings on the ocean bed. As the first big enterprise of this kind, it called for conservative design and the use of separate cables for the two directions of transmission. Subsequent experience, and the development of a deep-sea repeater capable of amplifying signals passing in both directions, will form the basis of British and American systems in the future. The new repeater, together with a new type of armourless cable for laying in deep waters, will be used for the Commonwealth Pacific project. This will give wire connexion for eighty telephone circuits all the way from London via Canada and New Zealand to Australia and a quality of transmission which should be equivalent to that on an inland call.

The new cables will not transmit television. The frequency band-width required for this would necessitate repeaters at very close spacing. This calls for the use of transistors instead of valves in order to simplify the problems which are encountered

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in supplying power to the repeaters from the shore ends of the cable.

The very rapid progress made during the past few years in space technologies has, however, opened the way to a new means of world-communication using satellites. The new systems will essentially be of wide-frequency band-width, providing facilities for television as well as for very large numbers of telephone circuits. Of these space systems those using active satellites are of the greatest long-term importance. In these the satellites will have to contain electronic equipment capable of receiving the radio signals and re-transmitting them back to Earth, as distinct from passive satellites which merely reflect the signals.

Two different kinds of system using active satellites have been proposed. First, those using a series of satellites which appear to move fairly rapidly across the sky at heights of between 2,000 and 6,000 miles. Second, those using satellites in circular equatorial orbits to a height of 22,300 miles. At this height the period of rotation is 24 hr. and a satellite appears stationary relative to a point on the Earth's surface. Three of them would give world-wide coverage for the relaying of radio signals. This has obvious advantages.

Many problems are being tackled, for example, in connexion with noise-free amplification of the very weak signals received back from the satellite, and in the provision of the necessary power supplies for the satellite-borne amplifier. But the techniques for doing these things are available or in an advanced stage of experimentation, and, provided means can be found for launching station-keeping satellites at reasonable cost, a commercial satellite communication system should be established in the not too distant future.

DATING MAN'S EMERGENCE

DR. K. P. OAKLEY chose as his presidential address to Section H (Anthropology) the subject of "Dating Man's Emergence". This is a problem that has excited curiosity since the earliest times of reflective thought, and attempts have been made to fix this in years, first by Biblical scholars such as Ussher who, using what were assumed to be historical data, calculated that the first man had been created in 4004 B.C., and then during the past hundred years the problem of man's antiquity has been investigated by prehistorians and geologists using scientific methods. The task of the prehistorians was to recognize the earliest artefacts or works of man, and of geologists to place these in stratigraphical sequence with reference to associated flora and fauna of prehistoric times. It was established early during the past century that man existed during the Quaternary Glacial or Pleistocene period, but the question whether he could be traced back into pre-glacial times, that is into the periods of the Tertiary era, has long been debated and is now recognized as partly a question of terminology.

Relative or stratigraphical dating has never fully satisfied our curiosity, and from early in the past century scientific attempts have been made to assess the duration of the geological periods in terms of years. Within this century they have in many cases been so dated by chronometric methods based on measurement of radioactive processes.

Before considering in detail the antiquity of man, it is useful to look at the whole evolutionary record of the order Primates, to which man belongs. We may distinguish six main points of emergence leading up to modern man (see table).

The earliest Primates were of the sub-order Prosimii, already in evidence at the beginning of the Palæocene, some 70 million years ago. The earliest known fossil representatives of the higher sub-order of Primates, the Anthropoidea, include what some authorities regard as primitive apes from Upper Eocene rocks about 40 million years old. The two most advanced families within this sub-order are the Pongidæ (apes) and Hominidae (men and their immediate antecedents). These are grouped together in the superfamily Hominoidea. Although the Pongidae have now been traced back to the Oligocene, and perhaps to the Upper Eocene, there is no evidence to suggest that the Hominidae, bipedal ground-dwelling Primates, were differentiated from apes (typically brachiating forest-dwellers) before Pliocene times. Possibly unspecialized apes of the genus *Proconsul*, predominant in Central Africa in mid-Miocene times (*c.* 15 million years ago), were not far removed from this important bifurcation in the evolutionary diversification of the Hominoidea. The emergence of the Hominidae has been placed hypothetically at the beginning of Pliocene times (*c.* 12 million years ago).

The oldest undoubted Hominidae were the Australopithecinae, with teeth of basically 'human' pattern, capable of living in open country, walking upright on two feet, but with brains not substantially larger than those of the great apes (*c.* 600 c.c.). They lived in Africa in Lower Pleistocene times, but precisely how early in this stage is a matter much under discussion. The question of whether the Australopithecinae should be counted as 'men' in the colloquial sense is also debatable.

The evolving hominids were distinguished from the pongids or typical apes by their dentition and by their ability to walk habitually on two feet, but there must have been some threshold at which their mental attributes became 'human' as generally understood. At one time it was widely held that this transition from 'animal' to 'man' occurred when in course of evolution the brain had expanded to a certain minimum volume, greater than that found in apes. This 'cerebral Rubicon' has usually been placed at *c.* 900 c.c., but a few authorities, including Keith, have put it at 750 c.c. (taking this as the mean volume within a population). Some neuro-anatomists, however, doubt any close correlation between brain size and mental ability; and an empirical definition of man is now favoured: man as tool-maker. Thus, wherever we find evidence that certain hominids were regularly making tools according to a set pattern, we may infer that they were capable of conceptual thought, and of establishing a tradition: such creatures were, I submit, 'men', whatever the size of their brains. In fact, Australopithecinae have been found at four or five sites in association with pebble-tools.

The earliest hominids with large brains (that is to say, brains exceeding the 'cerebral Rubicon' traditionally separating ape from man) are those of the *Pithecanthropus* group (for example, Java and Peking man), but none has been found in deposits earlier than those now classified as Middle Pleistocene (beginning with the Cromerian stage or Günz–Mindel interglacial in Europe). The pithecanthropines are sometimes grouped with neanderthaloid and modern man in the sub-family Homininae, the 'large-brained

men', as distinct from the 'small-brained men' of the sub-family Australopithecinae.

Dating in years the three main points of emergence in man's evolution (the first tool-makers, the first hominines, the first men of our own species) depends on establishing an accurate chronology of the Pleistocene period. Estimates of the duration of this period have varied enormously, partly on account of the lack until quite recently of chronometric methods applicable to its deposits, but also because of differences of opinion where to draw three important boundaries in the stratigraphical sequence: Pliocene/Lower Pleistocene, Lower/Middle Pleistocene and Middle/Upper Pleistocene. As now defined, the Pleistocene includes not only the Ice Age (glacial and interglacial stages, and in the tropics the corresponding pluvials and interpluvials), but also a stage largely pre-glacial known as the Villafranchian, distinguished by the emergence and spread of the three genera *Equus* (horse), *Elephas* (elephant) and *Bos* (ox).

The potassium/argon method of dating recently introduced has proved applicable to volcanic minerals in Lower and Middle Pleistocene deposits. The results so far obtained indicate that the oldest hominids, Australopithecinae in Bed I, Olduvai, are approximately a million years old, whereas the oldest hominines, in Asia and Africa, are about half a million years old.

Upper Pleistocene deposits are datable in some cases by the radiocarbon method (in practice limited to 60,000 years) and here the results indicate that the earliest known men of completely modern type, that is *Homo sapiens sapiens*, lived about 40,000 years ago, coming in at a time when the neanderthalers were dying out.

SUMMARIZING THE ANTIQUITY OF OUR ANCESTORS

	In millions of years
Earliest primates	70
Earliest Anthropoidea	40
Inferred emergence of Hominidae	12
Oldest tool-making Hominidae ('men')	1
Oldest hominines	½
Oldest examples of modern man	1/25

THE SENSES AND THE SCIENCES

THE sensory nervous system was the subject of Sir Bryan Matthew's presidential address to Section I (Physiology and Biochemistry). He pointed out that the experimental sciences have grown as man has developed means to push back the limits set by his sensory and motor equipment. The basis of the sciences is closely related to the properties of human nervous systems. Our increasing knowledge of nervous systems in animals and man in physical terms, enables this relation to be specified more clearly.

The state of knowledge of messages, in sensory nerves and their simpler reflex connexions is reviewed to uncover the nature of mechanisms they exhibit acting as predictors; these enable an organism to evade simple parts of physical causality in the environment. The high precision of responses to external stimuli is examined, directing attention to the significance of timing of nerve messages within the nervous system.

FROM GALTON TO CROWTHER

"WE are in crying want for a greater fund of ability in all stations of life; for neither the classes of statesmen, philosophers, artisans, nor labourers are up to the modern complexity of their several professions." It was with this quotation from Francis Galton that Prof. George Seth opened his presidential address to Section J (Psychology). Then, as now, it offers a natural starting point for a discussion of the contemporary world-wide demand for trained intelligence. It is appropriate also that, fifty years after his death in 1911, we should remember in Galton the last of the great Victorian scientists and one of the most distinguished members of the British Association. His name appears in its *Reports* nearly twenty times, thrice as sectional president; he was for four years general secretary, and twice unfortunate circumstances compelled his refusal of the presidency.

The psychology of individual differences stems from Galton's extension of Darwin, and psychometrics owes much to the statistical techniques that he developed. Because of his biological, hereditarian bias, his hope for a solution of the 'brain-power crisis' took a eugenic line. Current socio-psychological fashion looks rather to the educationist.

Terman's study of a gifted group 1,500 strong, from the childhood of 1922 to the middle age of to-day, bridges the biological-sociological gap between Galton and Crowther. Given a high enough intellectual pass-mark (*IQ*140), we can discriminate readily the children whose adult future will be distinguished. But not all will be outstanding, and even in such a highly selected group personal adjustment and cultural surroundings may intervene to reduce achievement.

The Terman findings are especially important to our contemporary educational revolution, which proposes a great extension of higher, and particularly of university education. In part simply continuing the democratization of education, the new demand arises more immediately from the pressure for economic expansion and military security.

The Crowther Report of the Advisory Council for Education concludes that there is much latent ability which has not declared itself in educational achievement. Too many boys, and proportionately still more girls, leave school early. The girls are, indeed, our largest reservoir of unrealized potential, and our administrative arrangements must be modified to enable us to make full use of the neglected talents of women. Here the problems remain of traditional restrictions on their employment, and justifiable concern about social return for educational investment. Having regard, however, to the use that industry makes of its graduate man-power, we may doubt whether we are ready in terms of attitude, or able, in terms of capacity, for the kind and degree of innovation rendered possible by an increased flow of educated people.

In the universities the supply of teachers is the principal bottleneck. But psychological research into the qualitative aspects of high-level mental functioning is certainly necessary, and our traditional or habitual teaching techniques seem ill designed to stimulate and foster its creative element. Moreover, many of the new undergraduates will be first-generation immigrants from grammar school. Both universities and schools, therefore, will have to cope with the problems of inadequate motivation and of dubious attitudes to knowledge and scholarship. Essentially this is a matter, not of courses and programmes, but of the quality of the contact between teachers and taught.

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PLANT TAXONOMY AS A SCIENTIFIC DISCIPLINE

DR. R. E. HOLTTUM chose as the title for his presidential address to Section K (Botany) "Plant Taxonomy as a Scientific Discipline". The great majority of species of higher plants occur in the tropics, and the classification of plants in temperate regions can only be understood if these are seen in relation to tropical plants. Study of the classification of tropical plants is now largely neglected at universities in Great Britain, in spite of the fact that national collections of specimens offer great scope for this work.

Monographs and Floras may appear to be complete, but where they are concerned with tropical plants most of them are in fact very incomplete; furthermore, being based on incomplete material, and often on very superficial examination of that material, the generalizations they contain are often invalid and sometimes quite erroneous.

The process of gaining a fuller understanding of the basic classification of tropical plants still rests on the classical procedures of taxonomy, which are not out-dated, though they appear to be out-moded. The only way in which to gain a fuller understanding is patient study of plants, but it does not consist merely in an accumulation of facts. Scientific progress of all kinds consists in the establishment of generalizations concerning accumulated facts, and such generalizations can only develop in minds which have been actively engaged in making observations. Traditionally, only certain parts of plants have been described by taxonomists, but new observation and thought may show that other parts are more important in gaining clear ideas of natural classification. To see clearly such hitherto unnoted structures, and to see the new knowledge so obtained in relation to the whole of existing knowledge, are acts of creative insight; and such acts are the essentials in the advance of taxonomy, as they are in any other branch of science.

In his address, Dr. Holttum gives examples taken from families of tropical plants which have been specially studied by him in Malaya, namely, ferns, orchids, gingers, bamboos and sedges, with references to the relation of British plants to those of the tropics. For example, the common fern *Polypodium vulgare* is seen as an isolated and reduced member of a great tropical family of epiphytes, namely Polypodiaceae in the strict sense, not the conglomerate still commonly called Polypodiaceae in text-books of botany; but the boundary of the genus *Polypodium* itself, now that other genera have been separated from it, needs new study to establish it clearly, and such study must be of tropical species. An understanding of the status of the genus *Dryopteris* (typified by *Dryopteris filix-mas*, the common male fern) is seen to involve a re-appraisal of the great family of tropical tree-ferns, Cyatheaceae, which has been most superficially treated by taxonomists, who in their turn have misled morphologists.

The native British representatives of the orchid family are highly specialized, and give no idea of the wealth and variety of species of orchids to be found in the wet tropics. British growers were pioneers in the cultivation of tropical orchids, and from them have produced a range of hybrids, carefully recorded, unmatched in any other family of plants; but British academic botanists have almost entirely neglected this valuable material. The only scheme of classification of the whole orchid family proposed in the present century was based on a scheme of 1889, and some of the basic concepts of that scheme certainly need a thorough revision; but such revision can only come from someone who has spent many years on a study of the whole family.

A very large amount of new observation, and of informed generalization, is necessary to give us a reasonably true picture of the classification of tropical plants. Meanwhile the native vegetation of the tropics is disappearing at an alarming rate, before we have an understanding of what the world is losing in the process.

EDUCATION IN UGANDA

"EDUCATION in Uganda" was the topic chosen by Dr. B. E. Lawrence for his presidential address to Section L (Education). Uganda is emerging from its long isolation from the rest of the world, and its younger people are learning other ways of life—particularly European ways—but the country remains very much a land of peasant agriculture with few wage-earners. As people do not live in villages but on their own 'shambas', it is extremely difficult to provide schooling for their children. The present system of education for Africans has grown out of the bush schools which were started by missionaries who sought to teach literacy as a means of spreading the Christian Gospel. To this day, nearly all the primary schools are mission schools—largely Catholic or Protestant—and newer schools, provided by the Government or by local authorities, are only just beginning to play a part in the system of education for Africans. There is a great deal of keenness for education, but parents' enthusiasm is less concerned with rudimentary literacy than with the utilitarian aim of helping their children to escape from the manual labour of subsistence farming.

Pressure for universal primary education is growing, and, as in other parts of Africa, may be promised by the political leaders before the means of providing schools or training the necessary teachers are available. There is a great dearth of education beyond the primary stage. Out of 1,000 children born in, say, 1950, perhaps 500 have been in primary schools for up to five years; 80 of these are likely to continue for two more years in a junior secondary school, 10 of whom will get four years of senior secondary education (of the grammar school type) from the age of fourteen, and 2 will go to a university—probably both of them boys. Perhaps in addition 8 boys and 2 girls of the 80 will go to a training college for teachers, where they will get some secondary education together with some professional training.

Most of the teaching of young children is in the hands of men, and, though many are good teachers, there is still far too much formal teaching by mechanical methods. A substantial proportion of older men are 'vernacular teachers' whose work is carried on in their tribal languages, of which there are many: six are recognized official languages.

English is taught after some years of primary schooling, and command of English is improving and becoming more usual. The English language is one of the most powerful unifying influences in the country.

Uganda needs much more money for education, and this is difficult in a country of subsistence farming where exports of primary products do not produce

a rapidly growing income. The country clearly needs help with capital expenditure on education, but above all it needs a supply of men and women from overseas, especially teachers and trainers of teachers who can work with the Africans when independence comes. Together they can bring into being a genuine African system of education; such a system must recognize the special needs of Uganda, a country which must succeed in agriculture in a modern world; the system must also gradually produce a professional class to undertake in all spheres the management of Uganda's affairs by the Africans themselves.

THE REVOLUTION IN AGRICULTURE

THE subject of the presidential address to Section M (Agriculture), given by Sir William Slater, was "The Revolution in Agriculture". In his opening remarks, Sir William directs attention to the fact that agricultural revolutions have their origins in economic and social causes, but the impetus which carries them forward is provided by the technical advances brought into use to meet the new situation. When the forces which have started a revolution have spent themselves or been contained, the revolution continues, if there are no further social or economic upheavals, until the new techniques have been absorbed into agriculture and a new stable system of farming has emerged.

The revolution through which we are now passing had its origins in the First World War. The threat to food supplies in Britain gave a great stimulus to agricultural research. Although farming went through a period of acute depression during the inter-war years, the research continued. The men were few and the equipment often inadequate, but the output of work was both considerable in quantity and high in quality. New techniques were developed and the old improved; most important of all, a store of fundamental knowledge was built up and was available as a basis for the solution of applied problems as they arose.

When war again called for a great increase in agricultural production, it was obtained, not by returning to the high arable farming of the nineteenth century, but by applying the techniques made available by research to develop an entirely new high level of production. The first step in increasing food production in 1939 was as in 1916, the ploughing of permanent pasture. From the outset, however, there was no intention to return to the high arable farming of the past, but instead a firm determination to move forward into a new scientifically based agriculture. Government and farmers alike looked to the raising of agricultural production not as a war-time measure only but as a pattern of farming for the future. While steps were taken for the immediate increase of production, it was always in the minds of those responsible at all levels that the ultimate objective was a steadily rising standard of efficiency. No limit in time or extent was set to this programme; it was tacitly accepted that efficiency would continue to rise so long as the scientists produced and the farmers applied the knowledge necessary to maintain steady progress.

The meaning of agricultural efficiency was not however defined. During the War, it meant the maximum yield almost regardless of cost. After the War the farmers still thought of efficiency as maximum production at guaranteed prices, while the Government began to regard efficiency in the terms of the lowering of costs and the limitation of subsidized production.

The full effect of the new techniques was not felt until after the Second World War, when yields rose steeply. This increased output was achieved with a diminished labour force and a high level of mechanization. The rise in output will continue, if no economic or political brake is put on the technical progress. It is not, however, a uniform rise over the industry as a whole. The progressive farmers are obtaining yields well above the average, while others are lagging far behind. British agriculture has reached a point of decision; it can either press forward applying every technique made available by science, and fit itself over the next decade to face any fair competition without subsidies, or rest at the present level of efficiency and trust to the continuation of subsidies for its future prosperity. If it takes the first road, the industry will have to be re-organized into larger units. The present structure, in which one man in three is a master, is quite unsuited to the rapid application of scientific methods, both in husbandry and in management. However such a re-organization takes place, it will be difficult and often painful, but an agriculture based on a high level of efficiency will have a far surer future than one which rests on political willingness to continue indefinite support by subsidies.

AFRICAN JURISPRUDENCE

MAJOR difficulties in the sociology of 'law' arise from controversy over the way in which this word of many meanings is used. It, and other unrelated words, have no one intrinsic, strict meaning; and criticism which is based on objection to another scholar using a particular meaning of the word is unjustified. These difficulties might be avoided if better use were made of the wealth of English, so that different phenomena—institutions, concepts, processes—could be distinguished by the use of different words, like legal, forensic, adjudication, litigation, etc., while perhaps retaining 'law' as a word of multiple meaning because of its general usefulness in analysis.

The problems of "African Jurisprudence", the subject of Prof. M. Gluckman's presidential address to Section N (Sociology), must then be broken into several different series, each distinguished by a separate word. Each of these series requires to be distinguished in order that we should be clear as to which one we are dealing with at any moment, and in order that we should be able to examine the effect of different kinds of phenomena on one another. It is particularly important to achieve this distinguishing clarity if we are to relate jurisprudential problems to the social relations in which they are involved, which is the main task of social anthropology.

For example, if we look at the study of the judicial process, we find that there are anthropologists who have insisted that this process in African tribes is so different from the similar process in the West that we cannot translate the concepts of Africans about this process into our own concepts. This would be cultural solipsism, excluding comparative generalizing. But if we examine the situations from which

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disputes arise for these tribal courts, we find that they are dealing with people who are permanently united in terms of status relationships, and that it is considered essential to try to reconcile the parties if possible. This affects the judicial process, but nevertheless the judge's main duty is to listen to evidence, cross-examine on and assess it by well-known rules, and give a verdict on it. This is apparent in the reports of those who insist that translation is distorting. Moreover, the process of reconciliation in similar relationships may be more marked in other Western procedures dealing with disputes in relationships involving many interests, as in lawyers' settlements of family disputes, or the conciliating settlements of industry. Nevertheless, these are different from court procedures, where authoritative verdicts are laid down.

The procedure of courts also differs from various processes of adjustment, by functionaries who range from champion-at-law through intermediary, negotiator, conciliator, to arbitrator in augmenting authoritativeness. The use of the wealth of English to distinguish these in varied terms, focuses attention on the process by which they work and the social relations involved, which are not well reported.

The argument that the concepts of Western law cannot be applied to African law has been stated in Bohannan's discussion of the fact that Tiv classify what we call actions in tort and in contract both as 'debt'. But so do the Kachins of Burma, the Bedouin of Cyrenaica, and other peoples: hence there is a general problem, to explain why many people at lower levels of technology fail to make this distinction. The answer may lie in the importance of property in status relations, and this can be handled only in a comparative, developmental morphology, of the type set by Maine and many other scholars. It seems possible to discuss the failure of tribal people to make this type of refinement in English with some special development of vocabulary, and more difficult to do the reverse.

The major contribution of social anthropologists in analysing the working of the total process of social control within their institutional setting cannot be examined here. This again is a problem distinctive from the others discussed. Finally, it is suggested that seances at which there is trial by divination, within a rational choice of alternatives between mystical agents, should be kept distinct from the judicial process. The search for mystical agents is again related to the types of social relations which occur in Africa: and therefore these situations of social control should be compared with similar situations in Europe, not with courts.

Obviously there are dangers that we may distort African law if we force it into English concepts, and we may have to specialize, adapt and refine our vocabulary. But those who insist on uniqueness block the advance of research and analysis.

SCIENCE, POLITICS AND SOCIAL SERVICE

IN his presidential address to Section X (General), the Earl of Cranbrook pointed out that during the earlier years of the historic period there seems to be a close relationship between the development of scientific and political thought. The Babylonians, acute observers of the movements of the heavenly bodies, unlike the Greeks, never seem to have wondered what those bodies were or what controlled their movements. There was a corresponding difference in the development of political thought in those two civilizations. After the Dark Ages, Europe was divided between educated priest and uneducated layman, and any major advance in scientific thought was retarded by religious intolerance. When, however, the main division came to be between rich and poor, the rich and educated being largely lay, scientific and political evolution could move forward together. The Royal Society, founded at a time when the educated man could be both philosopher and man of affairs, was in outlook what we would to-day call 'liberal', encouraging indirectly a sociological philosophy which led to all the advances of the following three centuries. At this point the discovery that science could only advance by 'experiments' planned so that the results were not affected by the preconceived ideas of the 'experimenter' led to the inevitable divorce between science and politics.

Men's thoughts and actions cannot be measured with accuracy or weighed on a balance. In spite of this, some laymen hope and some scientists maintain that, in a scientific world, trained scientists must know the answers to all political problems. This is no more true of scientists than of parsons, actors, authors or musicians—other professions where some members seem to think that because they are eminent artists or divines they are also the recipients of some revealed political truth. That is not to say that scientists and layman should drift apart. In Britain the foundation of the Royal Society was followed by the foundation of other societies both metropolitan, like the Society of Antiquaries and Linnean Society of London, and provincial, like the various philosophical, natural history and archæological societies which sprung up in provincial towns and counties. These enabled the educated rich and middle classes to understand the meaning and purpose of contemporary science and also to enjoy some of its pleasures. To these in the nineteenth century were added mechanics institutes and societies catering for the masses about to be enfranchized, so that as science developed so the more intelligent man in the street of all classes was able in some small way to keep in touch. To-day, television and radio programmes encourage a passive interest in science: somehow listeners and viewers must be encouraged to enjoy the pleasures of even minor scientific investigations. The British Association and local scientific and other societies, though they have done much already, have more work to do.

It would seem that the under-developed countries can learn much from our experience. The technical knowledge which it took us many centuries to achieve can be handed direct to them. The next step, that of teaching the man in the street to understand the meaning and purpose of science, which has taken us five or six generations, they will have to take in two or three at the most. They, too, will need their associations for the advancement of science and local archæological, natural history and philosophical societies. Many young people are going out to the under-developed countries, some as technicians, others to help with social service. They should go out already instructed in the history of those developments in their own country.

(*Continued from page 976*)

Table 1. *Nepytia* MORTALITY, NUMBERS OF INDIVIDUALS, AND NUMBERS OF EGGS DEPOSITED ON FIELD PLATE AND THE REST OF THE BOX, IN BOX I (CONSTANT FIELD), BOX II (INTERRUPTED FIELD), AND BOXES III AND IV (NO FIELD)

	Box I	Box II	Box III	Box IV
Mortality	3	1	1	0
Males	62	54	55	50
Females	36	44	43	48
Total	98	98	98	98
Total eggs	1,330	1,661	1,991	2,039
Eggs/female	36·94	37·75	46·30	42·47
Eggs on field plate	84	128	281	155
Eggs/female on field plate				
Upper surface	1·44	1·06	0·65	0·47
Under surface	0·88	1·84	5·88	2·75
Per cent on under surface	37·93	63·44	90·04	85·40

surfaces of the plates. This is probably related to the fact that *Nepytia* normally deposits most of its eggs on the under-surfaces of fir needles. In the boxes where an electrical field was present beneath the field-plate, however, and especially in box I where the field was continuously present, oviposition on the under-surfaces of the plates was conspicuously reduced. The field was very much more intense beneath the plate than above it, since the field was established between tray and plate. It was not possible to make a comparison of the numbers of eggs laid on the under-surface of the roofs of the boxes, since the numbers varied greatly between boxes.

These results show that the development of *Nepytia* pupæ, and also the number and location of eggs deposited by the adults, can be influenced by an electrical field. The magnitude of the experimental field was 180 V./cm. Fleming[5] states, "When the [thunder] cloud is within a few km., the pre-discharge field ranges from 10,000 to 30,000 volts per meter . . ." (p. 659). Thus, the intensity of the experimental field was not unreasonably high, compared with that found in Nature. However, a steadily maintained field of 180 V./cm. would probably not be found in natural atmospheric conditions, because fields associated with storms fluctuate markedly. The intermittent experimental field of 180 V./cm. in box II, which was somewhat closer to the natural situation, did not have as great an effect as the steady field. But the more intense experimental conditions were necessary in order to determine what kinds of effects to expect from application of a field, and what direction these might take.

I am indebted to W. G. Wellington and R. E. Foster of this Laboratory, for criticism of the manuscript and aid with the statistical analysis, and to H. S. Burr, Yale University School of Medicine, for his suggestions.

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MELANISM IN CAPTIVE WEAVER-FINCHES (*QUELEA QUELEA*)

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THE red-billed dioch, *Q. quelea*, is a weaver-finch which occurs in enormous numbers in Africa. Large, nomadic flocks roam the countryside in search of food and do enormous damage to crops and timber. The species has a wide distribution coinciding with the 30-in. rainfall isohyet[1]. It is sexually dimorphic: before the breeding season the male assumes a vivid nuptial plumage which temporarily distinguishes it from the permanently drab female (Fig. 1*a* and *b*). During the eclipse season, however, this cock plumage (black facial mask and light gold crown and breast) is shed and both sexes wear the same modest hen plumage, becoming morphologically indistinguishable.

During the course of a recent investigation of the breeding biology of this species[2], it was noted that some of the birds developed a black plumage. The birds, which had been flown from Africa, were housed in a large aviary under a constant photo-period of 12 hr. and a temperature of 65–75° F. Within three months of their arrival in London, some of the birds had become noticeably darker. In these specimens the entire plumage had been affected and became darker with each successive moult, until eventually the whole bird was black (Fig. 1*c*). The melanism occurred in both sexes and was common to eclipse and breeding periods.

Melanic males did not resume the typical nuptial dress during the breeding season and gave no external indication of coming into breeding condition Autopsy revealed, however, enlarged testes of maximum size filled with bunched spermatozoa. Female *Quelea* normally demonstrate a change of beak colour during the breeding season. In the contra-nuptial period both males and females possess bright red bills, but those of the latter sex change to pale yellow when the nuptial season starts. This colour cycle was unaffected by the assumption of a melanic plumage, and during the breeding period black females were easily distinguishable from melanic males by their yellow bills (Fig. 2*a* and *b*).

During their first year of captivity, about 8 per cent of the birds (about 40 specimens) became melanic. At the end of a further two years, the proportion of affected birds had risen as high as 30–40 per cent. Mr. D. C. Lourens, chief of the Division of Entomology, Department of Agricultural Technical Services, Pretoria, South Africa, has reported (personal communication) that 75 per cent of his captive *Quelea* have undergone a similar process of melanization to that described above.

So far as I am aware, melanic *Quelea* have never been seen in the wild and only appear as a result of captivity.

In order to investigate this phenomenon and determine what artificial factor or factors might produce it, groups of *Quelea* (50 specimens) were subjected to the following conditions:

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Photoperiodicity		
Group	I	8 hr. day-length
,,	II	12 hr. ,,
,,	III	17 hr. ,,
Temperature		
Group	IV	50° F. room temperature
,,	V	70° F. ,, ,,
,,	VI	95° F. ,, ,,

All the birds were fed on the same seed mixture and were kept under the above conditions for a period of four months (August–November), during which time they underwent a moult into breeding plumage. Groups I–III were all maintained at a constant room temperature of 70° F., and groups IV–VI had a uniform photo-period of 12 hr. In all groups the melanic birds were unaffected; but some of the specimens which had started in normal eclipse plumage moulted into a nuptial plumage showing signs of melanism.

In order to determine whether the quality (rather than the photoperiod) of the artificial light (fluorescent tube) may have been a contributory factor, advantage was taken of a period of mild sunny weather to expose melanic *Quelea* to sunshine. This resulted in the birds (four male specimens) reverting back to normal within the first moult. The birds were placed in an outside cage during the first week of May and all moulted to a normal eclipse condition.

One main difference in the quality of sunlight and artificial illumination is the reduction in the amount of ultra-violet in the latter. Recently, Lourens has been able to show that captive *Quelea* given a daily

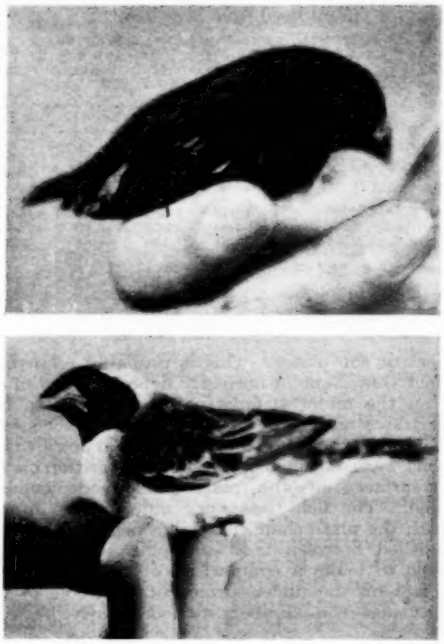

Fig. 3. Effect of exposure to ultra-violet light. (*a*) Melanic male before moult. (*b*) Same bird after exposure to ultra-violet light during moult

dose of ultra-violet light do not become melanic, whereas unexposed captive birds do. In the present investigation, 10 melanic *Quelea* were exposed to ultra-violet from an ordinary sun-ray lamp at a distance of 8 ft. from their cage. They were given two 8-min. sessions a day throughout one month while they were moulting. When feather regeneration had been completed, seven had assumed a normal nuptial dress (Fig. 3*a* and *b*), and three, though still melanic, had assumed a much lighter plumage.

It is not known how the ultra-violet rays exert their influences on pigmentation in growing feathers. The cyclical assumption of a breeding plumage in this and other weaver finches is under the direct control of the pituitary[3] and, in particular, is responsive to the output of luteinizing hormone[4]. This hormone cannot be responsible for the phenomenon described in the present work, however, since all the plumage is affected, whereas luteinizing hormone will only stimulate the deposition of melanin in those regions where it is normally found in the nuptial plumage, namely, the black facial mask. It seems not impossible that the ultra-violet influence may be imposed directly on the organizing feather follicle during growth of plumage.

I thank Mr. D. C. Lourens for his kindness in allowing me to refer to some of his unpublished results. This investigation was assisted by a grant from the Nuffield Foundation.

Fig. 1. (*a*) Male *Quelea* in nuptial plumage. (*b*) Female bird during breeding season. (*c*) Melanic female

Fig. 2. Melanic female in (*a*) eclipse period with red beak and (*b*) in breeding period with beak turning yellow

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LETTERS TO THE EDITORS

PHYSICS

Interaction of 14-MeV. Neutrons with a Silicon Semiconductor Nuclear Particle Detector

THE recent development of silicon semiconductor nuclear particle detectors has provided a convenient method for studying the charged particles emitted from the interaction of neutrons with silicon.

A silicon surface barrier detector, manufactured from 400-Ω cm. *n*-type silicon, was irradiated with neutrons of energy 14·03 ± 0·05 MeV. from the reaction T(*d*,*n*)^{4}He. Pulses from the detector, which was operated at a reverse bias of 50 V., were amplified by a charge-sensitive low-noise pre-amplifier and a modified 1430 Harwell linear amplifier and then analysed by a 400-channel *RIDL* pulse-height analyser. Fig. 1 shows a typical energy spectrum observed.

Assuming that reactions induced in silicon-28 (92 per cent abundant) are the most important then the possible charged-particle emitting reactions are ^{28}Si (*n*,*p*) ^{28}Al, $Q = -3{\cdot}86$ MeV.; ^{28}Si (*n*,α) ^{25}Mg, $Q = -2{\cdot}66$ MeV., and ^{28}Si (*n*,*d*) ^{27}Al, $Q = -9{\cdot}36$ MeV. The (*n*,*p*) and (*n*,α) reactions have been observed by Dearnaley and Whitehead[1] using 6·3-MeV. neutrons.

Calculation of barrier depth and comparison with range-energy values for protons and α-particles shows that unless the high-energy protons are emitted along the plane of the detector, they will escape from the sensitive volume of the detector whereas the maximum-energy α-particle range is much less than the barrier depth, and thus, apart from edge effects, all the α-particles emitted will be detected. Hence, one would expect a greater contribution to the observed spectrum from (*n*,α) than from (*n*,*p*).

Using the results of Hinds and Middleton[2] for the energy-levels of magnesium-25 and the energy-level scheme for aluminium-28 given by Endt and Braams[3], we calculated the expected energy of proton groups corresponding to the first seven levels in aluminium-28 and α groups corresponding to the first fifteen levels in magnesium-25. In Fig. 1, the arrows indicate the position of these groups and it will be seen that the position of the observed groups corresponds very closely with those expected from the (*n*,α) reaction and that there is little evidence for the (*n*,*p*) reaction under these conditions.

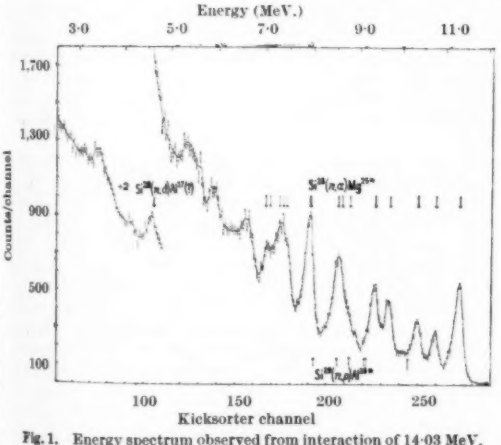

Fig. 1. Energy spectrum observed from interaction of 14·03 MeV. neutrons with a silicon surface barrier detector

The peak evident at 4·6 MeV. in Fig. 1 has been tentatively assigned on the basis of *Q*-value to the reaction ^{28}Si (*n*,*d*) ^{27}Al.

Cross-section measurements were made for the α-groups leading to the first five levels of magnesium-25. The results are given in Table 1.

Table 1. CROSS-SECTION FOR REACTION ^{28}Si(*n*,α) ^{25}Mg

Magnesium-25 (MeV.)	σ(*n*,α) at 14·03 MeV.
0	1·84 ± 0·61 mb.
0·582	0·75 ± 0·25 mb.
0·976	0·98 ± 0·33 mb.
1·612	1·19 ± 0·39 mb.
1·965	1·28 ± 0·43 mb.

Further work is in progress to study the (*n*,*p*) reaction and measure the excitation function for the (*n*,*p*) and (*n*,α) reactions.

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Magnetic Mirror Confinement of Exploding Wire Plasma

THE exploding wire phenomenon has recently been investigated by several laboratories as a high-density, high-temperature plasma source. For short durations temperatures in excess of 250,000° K. have been observed[1]. Further work has been carried out to confine the exploding wire plasma, making use of its very high current-densities and self-magnetic field[2].

It is the object of this communication to report the production and confinement of exploding wire plasmas in a magnetic mirror configuration. The high current and voltage needed for the exploding wire phenomena may also be used at the same time to produce the magnetic fields necessary for containment of the plasma. Because of the radial particle velocities released by the exploding wire[3], it is possible that a large number of ions will be trapped in an axial magnetic field. By this mechanism of producing a high-density plasma in the central region of a fast-rising, axial magnetic field, large densities of particles may possibly be achieved in the final confined state. In this experiment a capacitor bank of 1·7 μF. was

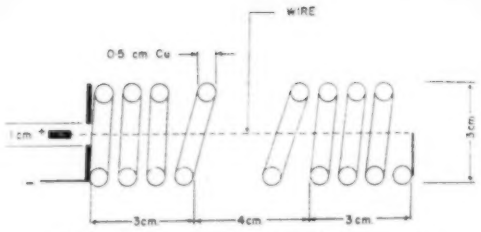

Fig. 1. Axial cross-section of magnetic mirror coil

initially charged to 22 kV. Connexions between the capacitor bank and magnetic mirror coil were made with flat sheet transmission lines, and the capacitor bank was triggered by a three-element spark gap. The capacitor bank has a rise-time of 4 μsec. after the initial triggering. The magnetic mirror coil consists of eight turns of 0·5 cm. diameter copper in the geometry as shown in Fig. 1. The wire to be exploded for producing plasma is shown in Fig. 1 to be on the axis of the coil in a series arrangement. All experiments were carried out in air at atmospheric pressure. The maximum current produced at the first current peak through the system, was estimated to be $1{\cdot}0 \times 10^4$ amp. This produces a maximum barrier field for the magnetic mirror coil of about 16 kilogauss, and a central field of about 7 kilogauss. A streak camera of the rotating mirror type was used for the observations of the plasma. For all photographs this camera was used at a writing speed of 13·2 μsec./cm., with the same film, at an optical stop of $f/2{\cdot}5$. The slit of the camera was focused on the central region of the magnetic mirror coil, perpendicular to the wire used for producing plasma. Streak photography was an effective means of recording the width of plasma with time.

Using $C = 1{\cdot}7$ μF., at 22 kV., and 3 mil copper wire with no external magnetic field, the exploding wire plasma shows the well-known current pause or 'dwell time'[4,5]. A streak photograph showing the 'dwell time' and 'restrike' appears in Fig. 2*a*. The beginning of the first explosion starts at $t = 0$ sec., and the 'restrike', or second current pulse, occurs at 75 μsec. During the time of the 'restrike' the light-emitting portion of the plasma is observed to increase in the radial direction to 4 cm. This pulse is then followed by an afterglow which continued beyond 210 μsec. The streak photograph in Fig. 2*b* was taken with the same conditions as Fig. 2*a* except that the plasma was produced in the central region of the axial magnetic field of the magnetic mirror coil. This streak photograph shows no 're-strike' or second current pulse. The plasma in the axial magnetic field is seen to increase in diameter to

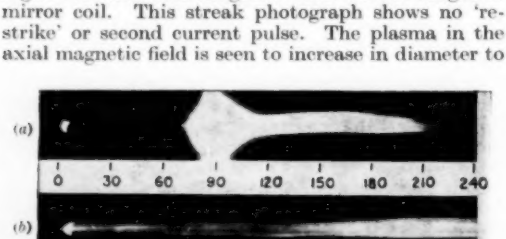

Fig. 2. Streak photographs of the radial motion against time of exploding wire plasma, *a*, with no external magnetic field; *b*, in the presence of an axial magnetic field. Time scale represented in μsec.

0·75 cm. in 7 μsec., where it then remains with a relatively constant diameter during the remaining 0·01 sec. existence of the plasma. The observed confinement time is found to be increased by 50 per cent by placing a 3 cm. copper disk, with a 1-cm. hole at the centre, at the negative end of the coil as shown in Fig. 1. This disk acted as a switch that shorted the capacitor bank once the plasma had expanded to 1 cm. diameter. Light intensity decreased considerably when placed in the axial magnetic field and streak photography was only possible when high-speed film was used with a fast lens. The appearance of longitudinal striations in the plasma (Fig. 2*b*), when placed in the magnetic field, may possibly be associated with the Kruskal–Schartzschild instability. The plasma never appears to break up but continuously decreases in light intensity until photography is made impossible. The possibility that the plasma may be forming a closed current loop with its own magnetic field running axially along the plasma may explain the long time of confinement from an initially pulsed system. Although most of the work done to date has been mainly qualitative the results warrant further investigation. In the future, magnetic probe studies and other observations will be made in high vacuum.

I thank Dr. J. D. Fox and Dr. E. A. Desloge for their helpful discussions and criticism, without which this work would have been impossible.

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Variation of Anisotropy Axis Dispersion with Thickness of Nickel-Iron Films

Thin films of nickel-iron evaporated in a magnetic field exhibit a uniaxial anisotropy. It has been postulated by West[1] and others that the anisotropy axes in local regions of the film vary in direction about an average axis. This angular dispersion β has been determined by West using magnetoresistance measurements and by Smith[2] with Bitter figure techniques. In the present work the variation of β with film thickness has been measured using an apparatus which has been fully described by Raffel *et al.*[3].

A field pulse H_A, greater than the anisotropy field H_K, was applied along the average difficult direction, and during the latter part of this pulse a small pulse H_D was applied along the average easy direction. The flux change parallel to the easy axis, due to the application of H_A, was measured with a single pick-up loop, and the minimum value of H_D giving full output was taken to be proportional to the maximum value of β. In practice, the approach to full output was too gradual for accurate measurement of this minimum value, and a level of H_D, H'_D say, giving 80 per cent full output, was used. An angle β′ given by $\sin\beta' = H'_D/H_K$ therefore gives the maximum dispersion in anisotropy over some 80 per cent of the film. Hence, 20 per cent of the film has hard directions at angles

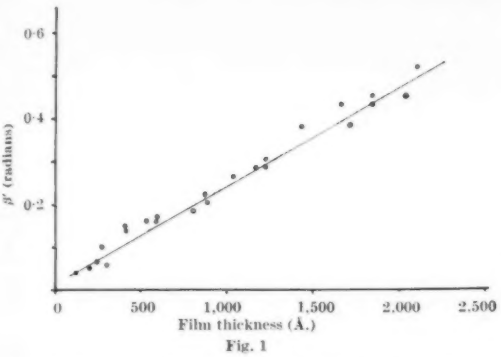

Fig. 1

to the mean greater than β′, and 80 per cent at angles less than β′.

The nickel-iron films used in one series of measurements were made in a single deposition, the thickness variation being produced by partially covering the ingot with a shutter. The alloy was deposited from an ingot heated by eddy current, on 0·008-in. thick glass substrates heated to a temperature of 300° C. in an aligning field of 10 œrsteds. The pressure duringn deposition was 10^{-5} mm. mercury, the nominal ingot composition was 82–18 per cent nickel-iron and the deposition took place in some 60 sec. The film thicknesses were measured using Tolansky fringes. The anisotropy field H_K was 3·0 œrsteds and independent of film thickness, while the coercivity H_c varied with thickness in a manner similar to that reported by Methfessel *et al.*[4].

Fig. 1 is a typical graph of β′ against film thickness from one such series and shows a linear increase of β′ from 0·04 radian (2·3°) at 120 Å. to 0·52 radian (30°) at 2200 Å.

Series of these measurements were also carried out using nickel-iron films from different depositions. The angle β′ always increased linearly with film thickness within the limits of experimental error; but the rate of increase varied from one deposition to another, showing that factors other than film thickness affect the angular dispersion. There is, for example, some evidence that β decreases with increasing deposition rate.

Bassett *et al.*[5] have observed an increase of grain size with increasing thickness for evaporated gold films, and a similar increase is to be expected with nickel-iron. Grain size, therefore, may well be the primary factor influencing β. If it is assumed[2] that the anisotropy of the films is caused by shape anisotropy of the grains or the associated anisotropic stresses, it follows that the angular dispersion of anisotropy could arise from a scatter in the orientation of these grains. The dispersion β was obtained by allowing the magnetization to rotate from a hard direction into one or other of the easy directions, forming domains of reversed magnetization. The dispersion of any small portion of film could therefore only be detected provided that its volume V were sufficiently large to form such a domain. If its volume were smaller than this, its magnetization vector would become aligned with that of the surrounding material. If the significant volume V contains grains each of volume v, one expects β to vary statistically as $\sqrt{\frac{v}{V}}$. Accordingly, β increases with grain size and hence with film thickness.

Work is now proceeding on the effect of the deposition-rate on angular dispersion and grain size.

I wish to acknowledge helpful discussions with Mr. R. S. Webley and Dr. E. P. Wohlfarth.

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SEISMOLOGY

Appearance of the *T*-Phase

Robson and Barr[1] have described the recording of bursts of seismic disturbances at Roseau on the Island of Dominica in The West Indies. The ground movement begins impulsively with a 0·3-sec. period oscillation and the whole disturbance lasts only for some 4 sec. They note that the tremors can be recorded only on the sand and gravel delta of the Roseau River, but because no local source for the tremors can be found they request the comments of other seismologists who find evidence of similar seismic phases.

In Tasmania, the University has installed a radio-telemetered seismic net[2] with a base station at Hobart and satellite stations at Moorlands, Tarraleah and Savannah. The geographical distribution of these stations is shown in Fig. 1. As was pointed out by Newstead and Watt[3] when discussing the Tasmanian net, the provision for central recording at Hobart prevents the occurrence of relative time differences

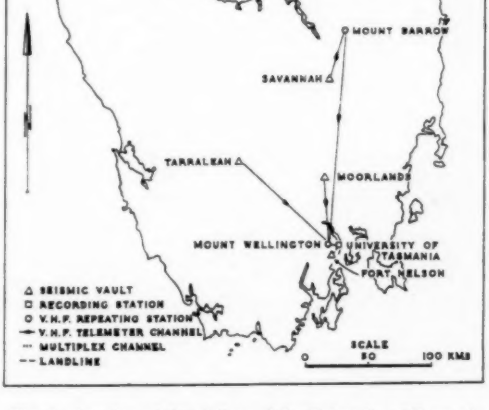

Fig. 1. Location of the stations of the University of Tasmania radio-telemetered seismic net

Table 1. Data for Two *T*-Waves arriving at Hobart from Milford Sound, New Zealand

Date	Epicentre	Origin time	Overall path		Water path		
			Distance (km.)	Travel time (sec.)	Distance	Travel time	Velocity
24.5.60	44·5 S. 167·5 E.	14 46 34	1,638	1,064	1,558	1050	1·48
25.5.60	44·0 S. 168·0 E.	02 37 42	1,691	1,092	1,611	1078	1·49

These seismic disturbances appear on the records as a small-amplitude, short-period tremor of limited duration. The overall epicentral distances and travel-times are given. The length of the 'Sofar' Channel which is at a water-depth of 1,500 m. is given together with the travel-time down the channel. The calculated velocity of 1·48, 1·49 is in agreement with the velocity of a compressional wave at a depth of 1,500 m.

between stations in the net. Consequently, an accurate measure of the velocity of a wave front across the continental crust of Tasmania as well as the velocity of the wave from the epicentre to Tasmania is possible.

Referring back to the appearance of the phase given by Robson and Barr[1] (see their Fig. 1), similar phases have been noted on the Tasmanian records and also on the records of other stations in eastern Australia (Doyle, H., private communication). We have identified the sources of some as due to natural earthquakes on the New Zealand side of the Tasman Sea and in other cases due to explosion of depth charges set off by naval ships. The mechanism of the transmission of the energy is as follows: From the earthquake epicentre or explosion point the energy travels as a compressional wave with a velocity of 1·47 km./sec. down the 'Sofar' Channel[4], which is a low-velocity wave-guide placed at an ocean depth of 1,500 metres. At the continental shelf conversion of energy from the channel wave phase to a *P*-phase and presumably an *S*-phase takes place. As recorded by a seismological station this *P*-phase appears to originate from a disturbance on the continental shelf.

In Table 1, the above hypothesis has been applied to two earthquakes in the Milford Sound region of New Zealand, which happens to be a prolific source of this type of wave. The channel wave can be seen to arrive 15–20 min. after the *P* wave and because of the long time-delay one may be deluded into thinking that the 'Sofar wave' or *T*-phase[5] is not connected with the initial seismic event in New Zealand. Furthermore, the wave front which forms on the continental shelf travels over the continent as a shallow *P*-wave. The late Fr. Burke-Gaffney[6] from Riverview Observatory, Sydney, examined 21 instances of this type of wave and he described them in the same terms as Robson and Barr; namely, bursts of small-amplitude high-frequency tremors. Brekhovskikh[7], in examining *T*-waves, found that the duration of the burst is dependent on the depth of water and the path-length over which the pulse travels. The constancy in appearance of the pulse as found by Robson and Barr suggests a fixed source of origin.

Byerly and Herrick[8], Dietz and Sheehy[9], and Wadate and Inouye[10] have all described the efficiency of the 'Sofar' Channel for the transmission of energy. A bathymetric map of the sea around the island of Dominica[11] shows that the 2,000-m. depth contour is very quickly reached off the coast of Roseau, thus facilitating the conversion of energy from the *T*-phase to the *P*-phase.

Shurbet[12] in 1955 examined the question of injection of energy into the 'Sofar' Channel, and he concluded that little more could be said quantitatively than to point out that the acoustical impedance across the boundary between the channel and the continent does depend on the structure of the continental shelf.

There still remains the problem of why the phase can only be positively picked up on the sediments of the delta of the Roseau River and not over the whole of Dominica Island. I would suggest that the acoustical impedance from sea water to delta sediments is very much less than from sea water to the volcanic rock of the island and as a consequence more energy is communicated to the ground in the vicinity of Roseau. Also with *P*-wave velocities as low as 0·75 km./sec. through the sediments of the delta, the amplitude of the ground movements would be enhanced, thus facilitating the recording of the phase with Willmore seismometers.

It would appear that the disturbances at Roseau are *T*-phases. These disturbances may or may not be preceded by body waves because, as pointed out by Byerly and Herrick, small earthquakes in Hawaii did not always give detectable *P* and *S* waves at California but the *T*-phase was an easily identified disturbance. If Robson and Barr could find evidence of small shocks somewhere in the Caribbean Sea[13] preceding the Roseau disturbances the problem is unambiguously resolved.

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METALLURGY

Transformation of Cathodically Impregnated and Cooled Austenite

The stabilization of austenite in two steels by hydrogen, introduced under certain special conditions, has been recently reported by us[1]. A similar but more pronounced stabilization has also been reported, by Gulyaev and Zelenova[2], for austenite monocrystals extracted by selective anodic dissolution. The stabilization can be satisfactorily explained[3], following Cohen *et al.*[4], on the basis of the de-activation

of the strain embryos in austenite by the migration and capture of hydrogen atoms, the driving force for such migration being provided by the electrolytic or pickling potential, resulting in the generation of hydrogen at pressures estimated to reach very high values. In these investigations, impregnation as well as removal of excess hydrogen by effusion, either naturally or deliberately, have been above the M_s temperatures of the steels, when they are wholly austenitic.

The effects of continuing the impregnation to temperatures well below the M_s point have now been examined, on a low-carbon, low-chromium tool steel (carbon, 1·1 per cent; chromium, 2·83 per cent) and a high-carbon, high-chromium die steel (carbon, 2·3 per cent; chromium, 13·6 per cent). These steels, with M_s temperatures of approximately 93° C. and 170° C., respectively, have been quenched from 980° C. into a 5 per cent sulphuric acid bath held at 98° C., cooled in it to room temperature (25° C.) while being cathodically impregnated and aged in air. The Vickers hardness on ageing has been determined, using, as in the previous work, a 30-kgm. load and taking all necessary precautions, the values reported being the mean of at least four perfect impressions. As a slight preparation of the surface of the specimen, after its removal from the electrolytic bath, is necessary for obtaining satisfactory hardness impressions, the first hardness measurement could be made only about 10 min. after such removal. The hardness values, which roughly indicate the extent of transformation, are shown in Fig. 1. The hardness increases appreciably within an hour of ageing, relative to the first measured value. The rise is more rapid in the initial stages and becomes virtually negligible after about 30 min. of ageing. It was unfortunate that hardness for very short ageing periods could not be recorded. However, from the recorded values, the extrapolated hardness value for an ageing period of about 1 min. appears to be substantially lower—of the order of 300–350 Vickers pyramid numbers (V.P.N.) only for the tool steel and 550 V.P.N. for the die steel. The higher value, in the latter steel, is due to cooling through nearly 80° C. in the martensite transformation-range before it is interrupted for impregnation, resulting in the retention at this stage of not more than about 25 per cent austenite, besides the high proportion of residual carbide present. This would also account for the lower over-all increase in hardness observed in this steel.

The most satisfactory explanation for these variations in hardness appears to be on the basis of the effect of pressure on phase transformation, in accordance with the le Chatelier principle. Hydrogen is generated during electrolysis at the surface of the specimen at enormous pressures. Its rapid penetration into the rifts, supposed to exist in steel[5], would further result in a considerable pressure being exerted on the microscopic portions of the austenite within the rift network. The austenite phase may therefore be expected to be considerably stabilized and the amount of martensite formed on cooling it to a fixed reference temperature to be considerably reduced, so long as electrolytic impregnation is continued and the hydrogen pressure maintained. However, as soon as the impregnation is stopped and the steel is removed from the electrolytic bath, hydrogen commences to effuse out, with release of the pressure, and the hitherto suppressed transformation may now be expected to occur in a manner

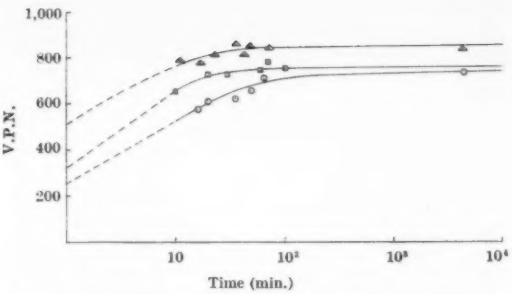

Fig. 1. Variation of hardness with time of impregnated and cooled austenite. ○ and □, Tool steel; △, die steel

roughly corresponding to the effusion of hydrogen. This would satisfactorily account for the rapid rise of hardness in the initial stages of ageing, followed by a virtually negligible increase, as most of the hydrogen effuses out within about 30 min., at room temperature, from a cathodically soaked specimen of the size employed by us.

Several authors[6–8] have examined the suppression of the austenite–martensite transformation by hydrostatic pressure. Patel and Cohen[6] estimate that the M_s point will be suppressed by about 5·7 deg. C. by a pressure of 1,000 atm. and experimentally have recorded a suppression of 8·5 deg. C. for 1,000 atm. Stregulin and Melnikov[7] have recorded a suppression of about 150 deg. C. for a pressure of about 20,000 atm. In view of the probably higher pressures prevailing during impregnation, it is quite likely that the M_s point in this investigation has been substantially depressed. Stregulin and Melnikov have also examined the subsequent isothermal transformation of austenite, the transformation of which to martensite is initially suppressed by pressure, at atmospheric as well as higher pressures, which are, however, lower than the initial suppression pressure. They indicate that in the first two or three seconds after the lowering of the pressure, the transformation proceeded very rapidly and slowed down after about ½ min. Such a very rapid initial transformation-rate may, however, be taken as improbable in our experiments, as although the effusion-rate is undoubtedly rapid, it is not so rapid as to result in the effusion of practically all the hydrogen in a few seconds. Pressure, during cathodic impregnation, is presumably exerted not only on the specimen as a whole from its exposed surface at which hydrogen is generated but also on microscopic portions of the specimen within the rift network, by hydrogen permeating into the rifts. Consequently, full release of pressure cannot be considered to follow instantaneously the removal from the electrolytic bath, unlike in the case of pressure application by classical techniques.

As the pressure of the cathodically generated hydrogen gas is dependent on the cathodic current density, it may be possible to study the austenite–martensite reaction at any value of hydrostatic pressure, merely by changing the conditions of electrolysis. The rate of release of pressure could also be probably controlled to some extent by controlling the rate of effusion of hydrogen out of the specimen—by refrigeration, for example.

Cathodic impregnation thus appears to provide a simple method of subjecting certain systems to high pressures, enabling their effects to be examined, under suitable conditions.

We thank Dr. B. R. Nijhawan, director of this Laboratory, for permission to publish this communication.

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MINERALOGY

Tetragonal Mullite-like Phase from Co-precipitated Gels

Much attention has been paid to the process of formation of mullite, a refractory mineral of the composition $3Al_2O_3.2SiO_2$, through decomposition of Al–Si clay minerals and crystallization of amorphous gels. In the course of my work on allophanes and related minerals[1], a new tetragonal phase was encountered by heating the co-precipitated Al_2O_3–SiO_2 gels at the temperature-range 910°–1,100° C.

The co-precipitated gels used in the heating experiments were prepared as follows: quartz powder was fused with sodium hydroxide and dissolved in the dilute aqueous solution of sulphuric acid containing an adequate amount of alum. The concentration of SiO_2–Al_2O_3 was kept constant (1 gm./l.) for all cases. Then an excess of hexamethylenetetramine (urotropin) was added in order to attain the homogeneous precipitation of SiO_2 and Al_2O_3. The precipitate was filtered, washed with water thoroughly until either potassium, sodium or urotropin showed no trace, and then dried at room temperature.

Chemical analyses were carried out to discover what extent the ratio of Al_2O_3 to SiO_2 in the original solution would be kept in the co-precipitation, and it was found that the ratio had not been seriously affected through the co-precipitation. Eleven different gels of varying Al_2O_3–SiO_2 ratios were thus prepared (Table 1). In X-ray diffractograms the precipitate showed no diffraction lines except amorphous pattern.

Each gel was heated for 5 hr. in air at different temperatures from 800° to 1,250° C. The sample was quenched in air and examined by X-rays. The result is shown in Table 1. A mullite-like phase was detected above 910° C. in gels of composition ranging from $SiO_2 : Al_2O_3 = 10 : 90$ to $80 : 20$. This phase is converted to an ordinary mullite above 1,250° C. In the composition-range higher in Al_2O_3 than $SiO_2 : Al_2O_3 = 20 : 80$, γ-alumina was formed at about 850° C., to be converted into corundum at 1,250° C. In the composition-range higher in SiO_2 than $SiO_2 : Al_2O_3 = 70 : 30$, cristobalite was formed at 1,250° C.

Table 1. Ratio of Al_2O_3 and SiO_2 Gels and Products by Heating

Percentage by weight SiO_2	Al_2O_3	Molecular ratio SiO_2	Al_2O_3	Products by heating for 5 hr. at ° C. 800	860	910	930	1,100	1,250
0	100	...	∞	—	*A*	*A*	*A*	*Co*	*Co*
5	95	1	11	—	*A*	*A*	*A*	*A*	*Co, Mr*
10	90	1	5	—	—	*A*	*A*	*A*	*Co, Mr*
20	80	1	2	—	—	*Mt, A*	*Mt, A*	*Mt, A*	*Co, Mr*
28	72	1	1·5	—	—	*Mt*	*Mt*	*Mt*	*Mr*
37	63	1	1	—	—	*Mt*	*Mt*	*Mt*	*Mr*
54	46	2	1	—	—	*Mt*	*Mt*	*Mt*	*Mr*
70	30	5	1	—	—	—	*Mt*	*Mt*	*Mr, Cr*
80	20	7	1	—	—	—	*Mt*	*Mt*	*Mr, Cr*
90	10	15	1	—	—	—	—	—	*Mr, Cr*
100	0	∞	...	—	—	—	—	—	*Cr*

—, Gel; *A*, γ-alumina; *Mt*, mullite-like phase produced at low temperature; *Mr*, ordinary mullite; *Co*, corundum; *Cr*, cristobalite.

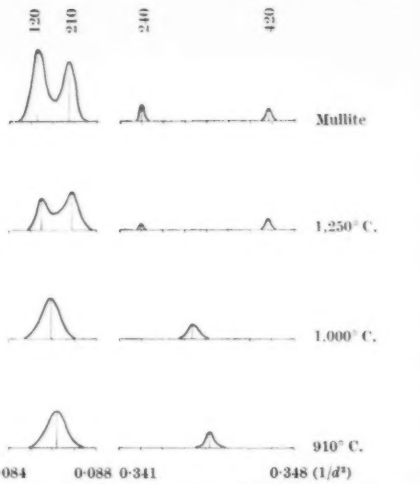

Fig. 1. X-ray diffraction pattern of products from heated Al_2O_3–SiO_2 gels and ordinary mullite

Of these eleven compositions given in Table 1, those yielding a mullite-like phase gave a sharp exothermic peak at 900°–1,000° C. in a D.T.A. curve while those yielding no mullite-like phase give no peak at the same temperature range[2]. Therefore, a close relationship between this exothermic peak in the D.T.A. curve and the appearance of the mullite-like phase might be indicated.

Further X-ray determinations were made on specimens of the composition $2SiO_2.3Al_2O_3$ heated at 910°, 1,000° and 1,250° C. with a Geiger-counter diffractometer. An arc-fusion mullite from an electrocast brick was examined for comparison. The mullite-like phase made at 910° and 1,000° C. gives generally fewer peaks than the ordinary mullite formed at 1,250° C. and arc-fusion mullite.

Table 2. Lattice Constants and Cell Volumes (v) of Products from Heated Al_2O_3–SiO_2 Gels and Ordinary Mullite

Products	a (Å.)	b (Å.)	c (Å.)	v (Å³.)	a/b	c/b
Heated at 910° C.	7·614		2·883	167·1	1·0000	0·3786
Heated at 1,000° C.	7·625		2·882	167·6	1·0000	0·3780
Heated at 1,250° C.	7·556	7·684	2·881	167·3	0·9833	0·3749
Arc-fusion mullite	7·565	7·672	2·883	167·3	0·9861	0·3758

Especially interesting is the fact that the pairs of 120 and 210 and of 240 and 420 reflexions of ordinary mullite[3] merge each into a single peak in the mullite-like phase as shown in Fig. 1. It was found that all the peaks of the mullite-like phase can be indexed reasonably in the term of a tetragonal lattice (Table 2). When plotted in the diagram unit cell volume versus c-axis, which shows clearly the interrelations in unit-cell dimensions of various mullites and silliman-

ites, this tetragonal mullite-like phase falls within the range of the 'iron free synthetic mullite' designated by Agrell and Smith[4]. When plotted in the diagram a/b versus c/b, this phase falls on the linear extrapolation of the line representing the 'iron free synthetic mullite'. There is no indication that this phase has any similar evidence to sillimanite.

All the evidence appears to indicate that this new phase has a very close similarity to mullite in crystal structure.

This communication reports work from Morimoto's Laboratory of this Institute. A detailed account is now in preparation.

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GEOLOGY

Two Unexplored Records of Quaternary History

During the last glaciation, world sea-level is generally thought to have been about 330 ft. (100 m.) below that of to-day. The rate of recovery for the last 200 ft. has been obtained from the (radiocarbon) age of peat beds and beach deposits which were covered successively by the rising sea[1]. Search for material to date the preceding 130 ft. could be made, quite reasonably, in a region of tectonic stability such as south-west England. The existing estuaries are too shallow to carry suitable material, but there are good prospects in the ground between the south coast of Ireland and the north coast of Cornwall. Here there is a hollow, the floor of which is 420 ft. below present sea-level, cut off from water of comparable depth, some 20 miles distant, by a sill the greatest depth of which is 330 ft. below present sea-level. Mitchell[2] considers that this sill and two others at a higher level in the Irish Sea are moraines, and this is supported by what is known of the composition of the sea-floor. Material from mud to stones is common. Pleistocene erratics certainly occur on the floor of the Irish Sea[3,4], and the height of the middle sill is far less than the thickness of the uncompacted materials, revealed by a seismic refraction station nearby[5].

If the southernmost sill is of Gipping age as Mitchell suggests, then it is likely that the hollow behind it was a freshwater lake during the last glaciation, during which the ice did not reach farther south than the two presumed moraines in the Irish Sea. The deepest sill was certainly land at some time for its surface is cut by two channels.

The proving of lake deposits could show that the sea had dropped below the level of the sill during the Pleistocene. The finding of a sequence of lake deposits or of peat should reveal when the sea-level rose to − 330 ft. Similar deposits in the Irish Sea could reveal when the sea-level rose to − 270 ft.

The two hollows are the sites of contemporary deposition of mud and sand, by the tidal currents, so that older materials will have been protected from erosion, at least locally, since they were covered by the sea. Search for the older materials will give a chance to assess the amount of recent deposition and therefore the importance of the supply paths that have been proposed[6].

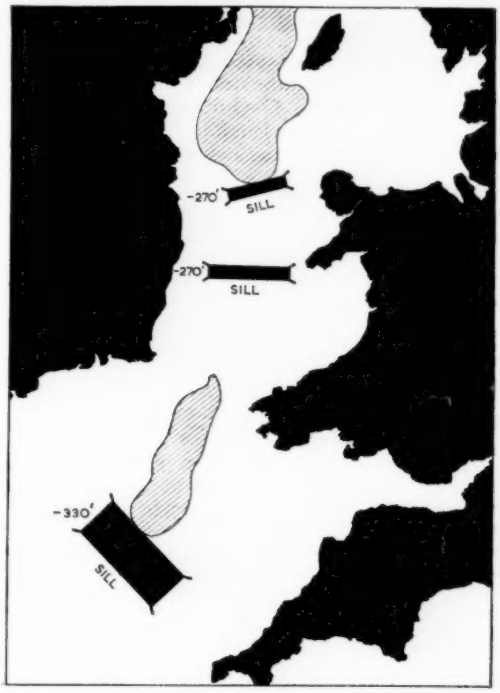

Fig. 1. The sills (? moraines) behind which freshwater lakes (diagonal shading) were probably in existence during the late Pleistocene

Examination of the northern hollow by means of the echo-sounder on R.R.S. *Discovery II* showed that there is up to 90 ft., of what experience shows can be interpreted as uncompacted material, lying partly on a rock floor. Sixty feet was proved by the same equipment in the southern hollow while a high-power, low-frequency echo-sounder, the 'Thumper', showed that the layer was underlain by other materials with a total thickness of 360 ft. (uncorrected for the velocity of sound in the material).

Further exploration is planned for the coming year, but it is anticipated that effective sampling of such sequences of deposits calls for ship-borne rotary drilling equipment of the type already used so successfully in deep water. Although such a technique is beyond the financial resources of the National Institute of Oceanography, it is hoped that the potential value of this suggested line of research will find wider support elsewhere.

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CHEMISTRY

Infra-Red Spectroscopic Evidence of Four-fold Co-ordination of Titanium in Barium Orthotitanate

Four-fold co-ordination of titanium has not been reported, either in inorganic titanates or in titanium oxides. Titanium is generally present as TiO_6 octahedra, although five-fold co-ordination has been reported in a few cases[1]. However, a study of titanium-bearing garnets led me to the conclusion that titanium (Ti) replaces silicon (Si), at least in part, and thus is four-fold co-ordinated in the garnet structure[2,3].

Further spectroscopic work on synthetic compounds has now given full confirmation of this interpretation and tetrahedral co-ordination of titanium has been ascertained, not only in solid solutions of the $X_2(Si,Ti)O_4$ type, but also in a pure orthotitanate Ba_2TiO_4, in $(Ba,Sr)_2TiO_4$ solid solutions and in some glassy structures.

First, the infra-red spectra of Zn_2SiO_4, Zn_2TiO_4 and $Zn_2(Si,Ti)O_4$ compounds will be briefly discussed. Zn_2SiO_4 has been chosen as a highly discriminative case because it has tetrahedral sites only. If some silicon (1–10 mole per cent) is replaced by titanium, two possibilities only are likely to occur: (1) Titanium does enter the willemite structure and is necessarily four-fold co-ordinated. The infra-red bands related to TiO_4 tetrahedra should be characteristic both from their frequency[3] and their pattern (the infra-red pattern given by fairly dilute solid solutions has been discussed briefly in a previous paper[4]). (2) Titanium does not enter the willemite structure and thus forms a separate compound for itself, namely Zn_2TiO_4, which is an inverse spinel with titanium in six-fold co-ordination. Other possibilities are unlikely, since no intermediate ternary compound is known in the system $ZnO - SiO_2 - TiO_2$.

The observed infra-red patterns are given as block diagrams in Fig. 1. I have interpreted[3] the infra-red spectrum of Zn_2SiO_4 as follows: 1,000–850 cm.$^{-1}$, stretching modes of deformed SiO_4 tetrahedra; 460–380 cm.$^{-1}$ bending modes of deformed SiO_4 tetrahedra; 620–570 cm.$^{-1}$ stretching modes of deformed ZnO_4 tetrahedra.

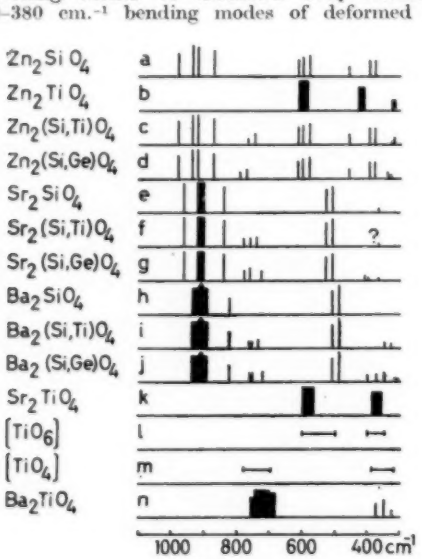

Fig. 1

No detailed interpretation is available for Zn_2TiO_4; the strong absorption centred near 590 cm.$^{-1}$ may well be due to either ZnO_4 tetrahedra or TiO_6 octahedra or both. Anyway, a detailed assignment is not needed for the present work, and the most important thing is the absence of strong bands at frequencies higher than 600 cm.$^{-1}$. This is in accordance with previous work on titanates[6], where TiO_6 octahedra have been found to absorb in the 600–500 cm.$^{-1}$ range.

The infra-red spectra of $Zn_2(Si,Ti)O_4$ samples with 1–10 moles per cent of titanium are characterized by two groups of new bands near 750 and 320 cm.$^{-1}$ (Fig. 1c), the intensities of which depend on the amount of titanium introduced; it is clearly impossible to attribute them to Zn_2TiO_4; moreover, their assignment to TiO_6 octahedra is inconsistent with the fairly high values 765 and 747 cm.$^{-1}$; as a matter of fact, this is within the absorption-range of GeO_4 tetrahedra as observed in orthogermanates (Tarte, T., unpublished results). On the other hand, GeO_4 tetrahedra in the $Zn_2(Si,Ge)O_4$ solid solutions gave a very similar infra-red pattern (Fig. 1*d*). This striking similarity is perhaps the most important reason for believing that titanium is also present in solid solution as TiO_4 tetrahedra.

If taken separately, each of these facts may be given several interpretations, but if considered simultaneously, they fully demonstrate that titanium has entered the willemite structure and is four-fold co-ordinated: the two groups of bands near 750 and 320 cm.$^{-1}$ are assigned to the stretching and bending modes respectively of slightly distorted TiO_4 tetrahedra.

Quite similar infra-red patterns have also been observed for other solid solutions such as $Sr_2(Si,Ge)O_4$ and $Sr_2(Si,Ti)O_4$, or $Ba_2(Si,Ge)O_4$ and $Ba_2(Si,Ti)O_4$ (Fig. 1, *e–j*), and this is again good evidence that a moderate amount of titanium may be introduced on the tetrahedral sites of these structures.

Let us now consider the case of pure Sr_2TiO_4 and Ba_2TiO_4. It turns out that these substances give quite different infra-red spectra.

In Sr_2TiO_4, titanium is six-fold co-ordinated and the observed infra-red spectrum (Fig. 1*k*) agrees with the general pattern of (TiO_6) octahedra (Fig. 1*l*) as observed in a series of metatitanates[6]. In Ba_2TiO_4, the detailed structure of which is still unknown, the strongest absorption is observed near 720 cm.$^{-1}$, a rather high value if compared with Sr_2TiO_4 or other structures with titanium in six-fold co-ordination. The infra-red spectrum of Ba_2TiO_4, however, agrees nicely with the characteristic frequencies of TiO_4 tetrahedra (Fig. 1, *m* and *n*). The same pattern is observed for $(Ba,Sr)_2TiO_4$ solid solutions (containing up to 10 moles per cent of Sr), which have essentially the same structure as Ba_2TiO_4. Further proof of the occurrence of TiO_4 tetrahedra has been given by the study of the composition $Ba_2(Ti_{0.95}Si_{0.05})O_4$. There is full spectroscopic evidence that silicon is present in solid solution as SiO_4 tetrahedra, thus confirming the occurrence of tetrahedral sites in the Ba_2TiO_4 structure. Tetrahedral co-ordination of titanium in Ba_2TiO_4 is thus definitely established. This is the first reported example of such a co-ordination in a titanate, but this result is in accordance with the suggested analogy of structure between Ba_2SiO_4 and Ba_2TiO_4 (ref. 8).

It should be pointed out that the absorption of TiO_4 tetrahedra in the 700–800 cm.$^{-1}$-region is fairly

strong and allows an easy discrimination between TiO_4 tetrahedra and TiO_6 octahedra. The method may be applied even to glassy structures, where I was able to detect spectroscopically both tetrahedral and octahedral co-ordination of titanium[9].

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Selectivity of Palladium Catalysts for the Oxidation of Carbon Monoxide in the Presence of Hydrogen

IT is known that a palladium catalyst used for the removal of traces of oxygen from hydrogen at room temperature is readily poisoned by carbon monoxide[1,2].

To remove oxygen or carbon monoxide from hydrogen containing both gases it is necessary, therefore, to work at higher temperatures. Catalysts recommended for effecting a selective oxidation of a few per cent of carbon monoxide in the presence of an excess of hydrogen are the metals of the eighth group of the Periodic System, especially platinum, rhodium and ruthenium[3].

These catalysts work selectively only between about 120° and 160° C.: below 120° C. poisoning occurs, above 160° C. the selectivity becomes lost.

W. J. Hanssen[4], when determining traces of carbon monoxide in the 5–200 p.p.m. range, applied platinum gauze at somewhat higher temperatures.

We carried out experiments with hydrogen gas containing only traces of carbon monoxide (50 p.p.m.) and found that in this case selective oxidation occurs already at room temperature. The results are given in Table 1.

Table 1. SELECTIVE OXIDATION OF CARBON MONOXIDE IN THE PRESENCE OF HYDROGEN ON 0·5 PER CENT PALLADIUM ON ALUMINA CATALYST (TOTAL PRESSURE 1 ATM., 50 P.P.M. CARBON MONOXIDE AND 0·2 PER CENT OXYGEN

Temperature (° C.)	Space velocity (h^{-1})	Conversion of carbon monoxide (per cent)
20	3,000	95
20	1,000	92
30	1,000	92
50	1,000	95
75	1,000	95

Throughout the period of investigation (several days) no indication of loss of activity was noted. The carbon monoxide was shown to have been oxidized to carbon dioxide and the oxygen content had been brought down to less than 0·1 p.p.m. An increase of the carbon monoxide concentration to 200 p.p.m. resulted in total poisoning of the catalyst. The poisoning was reversible.

The reactor used worked practically isothermally. The analysis was carried out with Hanssen's method (mentioned above) adapted to our lower concentration-range.

It can be concluded that removal of traces of carbon monoxide from hydrogen (necessary in some catalytic hydrogenation reactions) can be carried out already at room temperature.

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pH of Soap Solutions

THE specific conductivity and *p*H of sodium laurate solutions have been measured at several temperatures within a concentration-range of 10^{-1} M–3×10^{-5} M. The apparatus used was a modification of that used by Franks[1] for the chromatography of long-chain fatty acids. It enabled a concentrated (10^{-1} M) soap solution to be diluted continuously with rigorous exclusion of carbon dioxide at all stages, beginning with preparation of the soap.

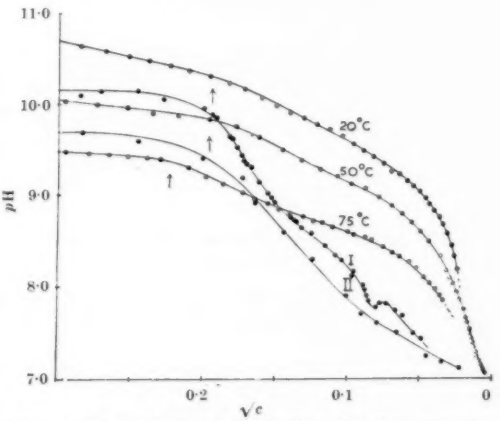

Fig. 1. *p*H of sodium laurate solutions. ○, This work; I, ref. 2 (20°); II, ref. 3 (25°)

The results are best presented graphically, and Fig. 1 shows the *p*H of sodium laurate solutions as a function of $\sqrt{c}$ at 20°, 50° and 75° C. The results of Ekwall[2] and McBain[3] are included for comparison. The present *p*H values are consistently higher than those of earlier workers and show very little scatter. The critical micelle concentration at each temperature is indicated by an arrow. Although the critical micelle concentration at 20° seems rather high compared with other published results, this could well be due to the total exclusion of carbon dioxide. Our value of $3{\cdot}4 \times 10^{-2}$ M is in excellent agreement with that obtained recently from e.m.f. measurements with membrane electrodes[4].

Fig. 2 shows the *p*H of a sodium laurate solution at 50° as a function of log c at extreme dilutions. At soap concentrations less than 10^{-4} M a linear relationship between *p*H and log c is obtained. The slope is in good agreement with the Debye–Hückel limiting law for uni-univalent electrolytes, and the soap behaves as a salt of a strong base and a weak acid. However, at concentrations greater than 10^{-4} M, marked deviations from ideal behaviour occur, possibly due to pre-association equilibria. The

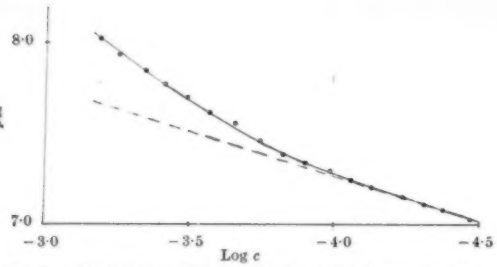

Fig. 2. *p*H of sodium laurate solutions at 50° C. over the low range of concentration. The dashed line represents the Debye–Hückel slope for a uni-univalent electrolyte

possibility of some form of pre-association below the critical micelle concentration has been suggested to account for the conductivity of sodium dodecyl sulphate[5], but the importance of such an effect has recently been discounted[6].

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Theoretical Expression for Solubility Product

While considering some questions on ionic interactions in a medium, I noticed a communication under this heading which includes an erroneous assumption[1].

Stating the potential energy of a 'system' of oppositely charged ions as:

$$U = \left\{ -\frac{(z_+\varepsilon)(z_-\varepsilon)}{R_0}\left(1 - \frac{1}{n}\right) - \frac{\alpha_a(z_+\varepsilon)^2 + \alpha_c(z_-\varepsilon)^2}{2R_0^4} \right\} \quad (1)$$

Krishnamurti derived an equation of the solubility product (equation 6) as:

$$-\log_e S_p = \left\{ \frac{\alpha_a z_+^2\varepsilon^2 + \alpha_c z_-^2\varepsilon^2}{2R_0^4} \right\} \Big/ RT + a \quad (2)$$

assuming that for very small values of R_0 the first term in (1), namely $\frac{-\varepsilon^2}{R_0}\left(1 - \frac{1}{n}\right)$, can be neglected in comparison with the second term representing the mutual ion-dipole interaction. (In equation 1, n is a constant which is approximately 9, α_a and α_c are polarizabilities of anion and cation respectively, z_+ and z_- are valencies of anion and cation, ε is the electronic charge and R_0 the equilibrium distance between the centres of the two ions.)

The assumption that, for very small values of R_0, the first term in (1) can be neglected in comparison with the second term is not correct. As the distance of closest approach of two ions can never be less than 10^{-8} cm. (except in the case of nuclear events), it can be seen that the first term can never be less than the second term. With α of the order of 10^{-24} cm.^{3}, $\varepsilon = 4{\cdot}803 \times 10^{-10}$ e.s.u., and with minimum value of R_0 is of the order of 10^{-8} cm., and with n and 9 it is seen that the first term is always greater than the second term at least by a factor of 10 for ions of different values of Z as 1, 2, 3, etc. Hence the first term can never be neglected in comparison with the second term. The first term, representing energy due to Coulomb interaction, is of course more important than that of the attraction resulting from the polarization of each ion in the field of the other as represented by the second term.

At constant temperature and with R_0 constant for compounds of similar constitution, a plot of $\log_{10} S_p$ against $\alpha_a z_+^2 + \alpha_c z_-^2$, Fig. 1 of Krishnamurti's communication, gave a straight-line graph, which led him to think that the derivation of equation 2 above was correct. But it should be noted that with n and R_0 constant (which point is also subject to question) the first term $\frac{z_+z_-\varepsilon^2}{R_0}\left(1 - \frac{1}{n}\right)$ itself becomes constant and thus equation 7 of that communication becomes:

$$-\log_{10} S_p = K(\alpha_a z_+^2 + \alpha_c z_-^2) + \lambda$$

where λ is another constant in which the contribution of the first term is included and thus a plot of $\log_{10} S_p$ against $\alpha_a z_+^2 + \alpha_c z_-^2$ would also lead to a straight-line graph. Thus a straight-line graph for $\log_{10} S_p$ against $\alpha_a z_+^2 + \alpha_c z_-^2$ does not justify the neglect of the first term, which, as I have explained, is more important than the second term.

I also wish to point out a discrepancy in equation 6 of Krishnamurti's communication (equation 2 here). By considering the interaction energy of two ions only, as in equation 1 here and not for all ions in a mole, Krishnamurti has arrived at equation 6 assuming his equation 3; in both equations RT there should be $\varkappa T$, where $\varkappa$ is Boltzmann's constant.

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[1] Krishnamurti, K., *Nature*, **182**, 1227 (1958).

When an ionizable compound BA is dispersed in water to form a saturated solution, equilibrium is attained with the rate at which the ions detach themselves from unit area of the surface of the solid particles (crystals) and go into solution, and is equal to the rate at which the ions recombine to form the molecules which are deposited on the same area of the surface. The rate of recombination of the oppositely charged ions $B^{\cdot}$ and A' to form BA, being deposited on unit area of the solid surface, is given by the expression: $k_1 n_{B^{\cdot}} n_{A'} . \exp(-E_1/RT)$, where $n_{B^{\cdot}}$ and $n_{A'}$ are the numbers of cations $B^{\cdot}$ and anions A' per unit volume of the solution; k_1 is a constant and E_1 is the energy per gm.-mole which the ions should possess to escape from the ionic atmosphere and from the solvation layers around the ions. This energy term was neglected in the earlier communication[1] and it should be correctly expressed as:

$$E_1 = N_0 \left\{ \frac{(z_+\varepsilon).(z_-\varepsilon)}{DR_1} + c\left(\frac{5}{9}\cdot\frac{A}{R_2^1} + \frac{7}{9}\cdot\frac{B}{R_2^2}\right) \right\} \quad (1)$$

where the first term represents the Coulomb attraction and the second[2] the energy of interaction between an ion of charge $z\varepsilon$ and c equidistant molecules of

Table 1

Compound	$N_0\left\{\frac{\alpha_a(z_+\varepsilon)^2+\alpha_c(z_-\varepsilon)^2}{2R_3^4}\right\}/RT$	$-\log_e S_p$	R_3 used in col. (2) (Å.)	R (from data in tables) (Å.)	$\gamma=\left(\frac{R_1}{R_3}\right)^4$
AgCl	22·58	22·58	2·788	2·77 (experimental)	1·47
AgBr	28·46	28·46	2·781	2·88 ,,	1·775
AgI	36·9	36·9	2·805	3·05 ,,	2·208
HgCl	40·2	40·2	2·431	2·434 (covalent)	2·351
HgBr	48·1	48·1	2·451	2·58 ,,	2·727
HgI	64·3	64·3	2·46	2·77 ,,	3·162
CuCl	13·66	13·63	3·154	2·77 (ionic)	0·595
CuBr	17·0	17·0	3·150	2·91 ,,	0·729
CuI	26·02	26·02	3·084	3·12 ,,	1·047
$HgCl_2$	33·54	33·57	3·293	3·01 ,,	0·698
$HgBr_2$	44·0	44·0	3·302	3·15 ,,	0·828
HgI_2	65·61	65·61	3·30	3·28 ,,	0·976
$PbCl_2$	9·21	9·21	4·728	3·13 ,,	0·192
$PbBr_2$	12·02	12·02	4·704	3·27 ,,	0·233
PbI_2	16·12	16·12	4·756	3·52 ,,	0·30

water at a distance R_3 from its centre ; $A=\frac{1}{2}\alpha(z\varepsilon)^2$; $B=z\varepsilon\mu$. N_0 is the Avogadro number ; z_+ and z_- denote the valencies of the positive and negative ions ; ε is the electronic charge, α the polarizability and μ the dipole moment of the water molecules; R_1 is the radius of the ionic atmosphere, and D is the dielectric constant of the medium.

The rate of re-dispersion of these ions from unit area of the surface of the solid BA in contact with the saturated solution is $k_2.n'.\exp(-E_2/RT)$, where n' is the number of pairs of ions (B^- and A') per unit area of the surface, and this re-dispersion of ions will take place when they possess energies exceeding E_2, which is the energy due to the attraction caused by Coulomb forces and the mutual polarization of ions:

$$E_2=N_0\left\{\frac{A_0(z_+\varepsilon)(z_-\varepsilon)}{R_3}\left(1-\frac{1}{n}\right)+\frac{\alpha_a(z_+\varepsilon)^2+\alpha_c(z_-\varepsilon)^2}{2R_3^4}\right\} \quad (2)$$

where A_0 is the Madelung constant, n occurs in Born's repulsion energy term, α_c and α_a are the polarizabilities of the ions, R_3 the interionic distance in crystals. In addition to these interactions it may sometimes be necessary to consider the dispersion forces between the ions, which may be of significance at close distances of approach of the ions characterized by higher polarizabilities. At equilibrium:

$$K_1'(N_{B^-}N_{A'}).\exp\left[-N_0\left\{\frac{(z_+\varepsilon).(z_-\varepsilon)}{DR_1}+c\left(\frac{5}{9}.\frac{A}{R_2^4}+\frac{7}{9}.\frac{B}{R_2^2}\right)\right\}\right]/RT$$
$$=K_2'N'.\exp\left[-N_0\left\{\frac{A_0(z_+\varepsilon).(z_-\varepsilon)}{R_3}\left(1-\frac{1}{n}\right)+\frac{\alpha_a(z_+\varepsilon)^2+\alpha_c(z_-\varepsilon)^2}{2R_3^4}\right\}\right]/RT \quad (3)$$

It was stated in the previous communication[1] that for small values of R_3 the term $\left(\frac{(z_+\varepsilon)(z_-\varepsilon)}{R_3}\right)$ can be neglected in comparison with the second term representing the mutual ion-dipole interactions. As pointed out by Dr. B. N. Bhar, this is not correct for the commonly accepted values of inter-ionic distances. This error did not, however, invalidate the main expression (5) or the approximate expression (6) given earlier[1]. This term may be omitted from the final expression when its numerical value is of the same order of magnitude as the term:

$$\left\{\frac{(z_+\varepsilon)(z_-\varepsilon)}{DR_1}+c\left(\frac{5}{9}.\frac{A}{R_2^4}+\frac{7}{9}.\frac{B}{R_2^2}\right)\right\}$$

occurring on the left side of equation (3). Then we may write (3) in a simpler form:

$$-\log_e S_p=-\log_e(N_{B^-}.N_{A'})=N_0\left\{\frac{\alpha_a(z_+\varepsilon)^2+\alpha_c(z_-\varepsilon)^2}{2R_3^4}\right\}/RT+a \quad (4)$$

where $N_{B^-}, N_{A'}$ relate to gm.ions and a is a constant. This is almost the same expression which was obtained previously[1]. Its validity is revealed by results given in Table 1. The agreement between the experimental values of $-\log_e S_p$ and those calculated from the expression (4) using the values of R_3 given in column 4 is quite good. If we use instead the interionic distances (R_1) obtained from the usually accepted values of ionic radii in this expression, we have to multiply it by a factor $\gamma=\left(\frac{R_1}{R_3}\right)^4$. There is a general proportionality between γ and the extent of covalent bonding in the corresponding halides. Thus, expression (4) should be written in a more general form as follows:

$$-\log_e S_p=\beta N_0\left\{\frac{\alpha_a(z_+\varepsilon)^2+\alpha_c(z_-\varepsilon)^2}{2R_3^4}\right\}/RT+a \quad (5)$$

where β corrects for the covalent contribution to the bond between the ions in the crystal, which causes a decrease in the solubility of the salt. In fact, copper, silver and mercury show considerable tendency to form covalent compounds, and their halides (except fluorides) show very low solubilities.

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BIOCHEMISTRY

A New Family of Potent Carcinogens : Benzopyridocarbazoles

Although the introduction of heterocyclic nitrogen atoms in the molecules of polycyclic compounds sometimes decreases carcinogenic activity (for example, the inactive pyrenoline as compared with 3,4-dibenzopyrene[1]), there are many instances in which the carcinogenic potency is thereby main-

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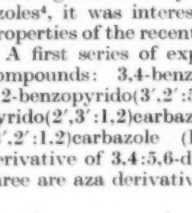
tained or even enhanced. Such is the case with the angular benzacridines[2], some of which are more active than the corresponding 1.2-benzanthracenes in respect of induction of skin epitheliomas and sarcomas, and with the inactive pentacene, in the molecule of which the introduction of two nuclear nitrogen atoms leads to the sarcoma-producing dinaphthazine[3]. In view of the already established carcinogenic activity of the angular dibenzocarbazoles[4], it was interesting to examine the biological properties of the recently synthesized aza derivatives[5].

A first series of experiments concerned four such compounds: 3,4-benzopyrido(3′,2′:5,6)carbazole (I), 1,2-benzopyrido(3′,2′:5,6)carbazole (II), 5,6-benzopyrido(2′,3′:1,2)carbazole (III), and 5,6-benzopyrido-(3′,2′:1,2)carbazole (IV). The first is an aza derivative of 3,4:5,6-dibenzocarbazole, and the other three are aza derivatives of 1,2:5,6-dibenzocarbazole.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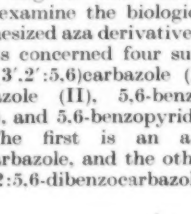
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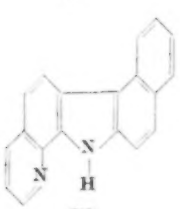
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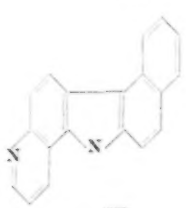
(IV)

Using male and female mice aged 4–6 months and belonging to strain XVII *nc/Z* (Radium Institute), the substances were injected subcutaneously in the flank (3 injections of 0·6 mgm. of the substance in 0·2 c.c. olive oil, with an interval of one month between injections). The results are recorded in Table 1; no hepatic lesions were observed in any of the mice.

Table 1

Substance	No. of animals	No. of sarcomas	Average latency (days)	Sarcoma index (after Iball)
I	14 ♂	1	106	13
	♀	0	—	0
II	14 ♂	2	273	10
	♀	1	200	7
III	16 ♂	8	157	64
	♀	1	178	7
IV	15 ♂	7	100	100
	♀	6	118	73

From these results, it can be seen that the group of aza derivatives includes carcinogens with various degrees of activity, some of them being extremely potent. Compound (IV), for example, ranks among the most powerful sarcomagenic substances known, with both a high proportion of tumours and a strikingly short induction period. A shift in the position of the pyridinic nitrogen atom can diminish this activity either slightly (compound III), or considerably (compound II). Hence, it is clear that, as in the benzacridine series, biological activity is to a large degree dependent on the position occupied by the pyridinic nitrogen atom; and this opens the field for a broad investigation into relationships between structure and activity in this new chemical series, an investigation which is now being pursued here.

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Amino-Acid Activation in Tissues of Early Embryos

The work of Hoagland, Zamecnik and many others[1–4] has now clearly established that activation of the carboxyl groups of amino-acids occurs as one of the initial steps before they are incorporated into proteins during protein synthesis. The activation reaction was demonstrated by these workers, in protein-synthesizing components of a variety of mammalian tissues. Recently amino-acid activation has also been shown to occur in sea-urchin eggs, but I have seen no reports to date of any investigations on tissues of developing embryos.

The work to be reported briefly here shows that amino-acid activation can be detected in embryos of the clawed toad, *Xenopus laevis*, and it also deals with the occurrence of leucine-activation in tissues of both *Xenopus* and chick embryos. Fuller details of the work on *Xenopus* are in process of publication[4]. Amino-acid activation has been measured as an increased rate of exchange of phosphorus-32 between radioactive pyrophosphate (PP) and adenosine triphosphate (ATP) in the presence of added amino-acid, as compared with the exchange-rate without added amino-acid. The principles and details of the method have already been described[4–6]. Supernatants from tissues homogenized in ice-cold 0·05 *M* potassium chloride at *p*H 7·5 and centrifuged in the cold at 20,000*g* have been used as sources of enzyme.

Table 1 gives the results of testing supernatants from hatching-stage *Xenopus* embryos with a number of different amino-acids. It is apparent that all these show activation, that is, the PP–ATP exchange-rates are higher than when no amino-acid is added (controls). The most marked activation is shown for tryptophan, tyrosine and leucine. In later work[4] on the distribution of leucine-activation in dorsal and

Table 1. Enhancement of PP–ATP Exchange by Various Amino-acids, in Extracts of Hatching-stage *Xenopus* Embryos

Amino-acid added (2·0 μmoles)	% PP–ATP exchange*
None (controls)	0·751
Serine	3·265
Threonine	3·478
Glutamine	3·736
*Iso*leucine	4·276
Tryptophan	5·669
Tyrosine	4·341
Leucine	6·171

* $\%\text{Exchange} = \dfrac{\text{specific activity of ATP (counts/min. μmole)}}{\frac{1}{4}\text{ specific activity of PP (counts/min. μmole)}} \times 10$

Table 2. LEUCINE-ACTIVATION IN DORSAL TISSUES OF *Xenopus* EMBRYOS, AT LATE NEURULA TO EARLY TAIL-BUD STAGES
Activation expressed as percentage increases in PP–ATP exchange-rate in the presence of 2·0 μmoles of L-leucine, per 10 μgm. total nitrogen in supernatant sample. Means and standard errors of five experiment

	% increase in PP–ATP exchange
Somite tissue	34·3 ± 9·2
Nervous tissue	25·7 ± 7·8

ventral regions of embryos, it was found that dorsal halves of late neurula and tail-bud stages had a particularly high leucine-activating activity, and that when these dorsal halves were divided into somite and central nervous tissue portions, the highest leucine-activation was shown in the somite tissue (Table 2: the activation figures are here expressed as percentages and related to total nitrogen in the enzyme samples). The difference between somite and nervous tissue was highly significant ($t = 4{\cdot}9954$, $P < 0{\cdot}01$ for the paired differences between values for somite and nervous tissue in five experiments).

The same method has been used for measuring leucine-activation in somite and central nervous tissues of 48-hour chick embryos (stage 11–13 of Hamburger and Hamilton's series[7]). The only methodological difference was that the volumes of all reagents in the incubation mixture were reduced to a quarter of those used for the *Xenopus* extracts, since the total bulk of chick tissues available was very much smaller (the total nitrogen in chick supernatants was only one-tenth of that in *Xenopus* supernatants). The results with chick tissues were far more striking than those with *Xenopus*, and increases of PP–ATP exchange of more than 100 per cent were obtained (Table 3). Again, the leucine-activation in somite tissue was higher than in central nervous tissue ($t = 8{\cdot}2975$, $P < 0{\cdot}01$, for the paired differences between values for somite and nervous tissue in five experiments).

Table 3. LEUCINE ACTIVATION IN TISSUES OF 48-HR. CHICK EMBRYOS
Percentage increases in PP–ATP exchange-rate in the presence of 0·5 μmole of L-leucine per μgm. total nitrogen in supernatant sample. Means and standard errors of five experiments

	% increase in PP–ATP exchange
Somite tissue	181·89 ± 29·83
Central nervous tissue	100·64 ± 29·45

The specially high leucine-activation in somite tissue extracts of both *Xenopus* and chick embryos suggests that more leucine is being taken up into proteins of somite cells at this particular stage of development than elsewhere in the embryo. As we are dealing, in both types of embryo, with stages during which the somites are segmenting, it is of interest to note that, in chick embryos, somite segmentation can be blocked by analogues of leucine[8,9], or, conversely, speeded up by adding normal leucine to the culture medium[10]. It is not at all clear what role leucine could have in controlling the process of somite segmentation. One might, however, speculate that somitic myoblasts are prime users of this amino-acid, at least during the time that they are synthesizing myosin, for this protein contains a high proportion of leucine[11]. Holtzer, Marshall and Finck[12] have detected myosin in chick skeletal myoblasts at stages only slightly later than those used here, that is, about 56 hr. incubation (stage 16, 17). So it is reasonable to suggest that at stages 11–13 leucine could already be being taken up into myosin precursors. Autoradiographical work is needed to elucidate this point, and some has already been started by me.

Returning to the evidence (Table 1) that in late *Xenopus* embryos activation of a number of amino-acids can be demonstrated, this is of general interest because one should certainly expect embryonic tissues which are differentiating to be carrying out very active protein synthesis. It would seem that we have here a promising and wide-open field for further study.

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Changes in the Pentose-Nucleic Acid Content of Sub-cellular Fractions of the Brain of the Rat during 'Metrazol' Convulsions

CHANGES in the content of pentose-nucleic acid in the nerve cells during increased functional activity have been reported by several workers. A depletion of pentose-nucleoprotein in the stimulated anterior horn cells has been observed by Holger Hyden[1]. Similarly, a decrease in the concentration of pentose-nucleic acid, together with an alteration of molar base ratios, has been reported in the stimulated area of the brain cortex in cats[2]. A consistent decrease in pentose-nucleic acid and protein content of the brain of the rat during 'Metrazol' convulsions has been observed and reported elsewhere[3].

In the present communication the changes in pentose-nucleic acid of various sub-cellular fractions of brain of the rat during convulsions are presented.

Convulsions were induced by injecting 'Metrazol' into albino rats[4], keeping litter-mates as controls. The animals were decapitated in the tonic phase of convulsions and the heads frozen by dropping them in liquid air. The frozen brains from two animals were pooled, quickly weighed and the nuclei separated by the method of Alfrey, Osawa and Mirsky[5]. Microsomal and *p*H 5 pentose-nucleic acid fractions were separated by the methods of Osawa, Takata and Hotta[6] and of Schweet, Lamform and Allen[7] respectively. From the sucrose nuclei, the *p*H 7·1 soluble pentose-nucleic acid fraction and the nucleolar pentose-nucleic acid fraction were separated by the method described by Alfrey and Mirsky[8]. From the fractions thus obtained pentose-nucleic acid was extracted and estimated according to the micro method of Scott, Fraccastoro and Taft[9].

Table 1 summarizes the net change in each of the sub-cellular fractions. It is apparent that the depletion in the content of pentose-nucleic acid during convulsions is primarily confined to the nuclear fractions. Ruth S. Geiger[10] has observed in histological studies that an extrusion of the nucleolar material occurred

Table 1. Pentose-Nucleic Acid (PNA) Content of Various Subcellular Fractions in Normal Rat Brain and After 'Metrazol' Convulsions

PNA fraction		Control	'Metrazol' convulsive	Percentage of change
Nuclear	Soluble Nucleolar	6·4 ± 0·73 (12)	4·8 ± 1·04 (12)	−25
		27·4 ± 4·4 (12)	20·6 ± 4·5 (12)	−24
Cytoplasmic	Soluble	22·5 ± 3·6 (12)	21·8 ± 5·0 (12)	− 3·1
	Microsomal	52·6 ± 5·2 (10)	49·4 ± 5·3 (12)	− 6
Total	Nuclear Cytoplasmic (excluding mitochondrial)	33·8 ± 4·4 (12)	25·5 ± 4·0 (12)	−24·5
		75·6 ± 9·6 (10)	71·3 ± 6·3 (12)	− 5·6
Total (cytoplasmic + nuclear)		107·8 ± 15·8 (10)	96·8 ± 13·6 (12)	−10·2

The figures in parenthesis indicate the number of animals used. The brains of two animals were pooled together for obtaining a single observation. Values are expressed in μgm./100 mgm. wet weight of tissue. Against each value, the mean standard error is given.

in neurons in tissue culture when subjected to electrical stimulation or 'Metrazol' action.

This fall in nuclear pentose-nucleic acid may be due to an enhanced activity of ribonuclease in the nucleus as a result of convulsive activity or it might indicate an increased passage of pentose-nucleic acid from the nucleus to the extra nuclear compartments in convulsive or electrically stimulated states. Relationship of these changes with the functional activity of the neurons is not yet defined.

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Rapid Infra-Red Determination of Acetone in the Blood and the Exhaled Air of Diabetic Patients

There has long been a need for a rapid method for the quantitative determination of acetone in the blood and in the exhaled breath of the patient with diabetes mellitus. Since these concentrations are a direct reflexion of the diabetic state, precise acetone measurements would provide the physician with more accurate data with which to evaluate his diabetic patient. To be clinically practical, this analytical method should be specific, sensitive and rapid. An extension of an infra-red method previously reported[1] provides the basis for this preliminary report on the analysis of acetone in blood and in the expired air.

To determine the acetone in blood, a 10-ml. oxalated sample is pipetted into a 40-ml. glass centrifuge tube fitted with a ground-glass stopper. 15 ml. of carbon disulphide is added, and the tube gently shaken for 10 min. on an automatic shaker. The tube is then centrifuged in a refrigerated centrifuge for 5 min. at 1,500*g*. The solvent layer is transferred to a 15-mm. infra-red sample cell and the absorbance at 8·25μ measured. To compensate for solvent absorption a matching cell filled with carbon disulphide is placed in the reference beam of the spectrometer. The minimal amount of acetone detectable in blood by this solvent extraction technique is 1·5 mgm. per cent (15 p.p.m.). The standard deviation in the analysis of a sample of blood containing 13·7 mgm. per cent of acetone is 0·76 mgm. per cent, while that of a 61·4 mgm. per cent acetone in a blood standard is 2·2 mgm. per cent.

Expired air for acetone analysis is collected by having the diabetic patient slowly exhale into a 10-l. 'Saran' bag. This expired air is allowed to flow into an evacuated gas cell of path 40-m. long mounted on a Perkin–Elmer model 221 infra-red spectrophotometer. The absorbance at 8·25μ is measured to determine the concentration of acetone. The minimal amount of the compound detectable is 10 μgm./l. of expired air. The standard deviation in an air sample containing 600 μgm./l. is 31 μgm./l.

The infra-red analysis of blood acetone has proved to be clinically practical. It provides a precise measurement of acetone in the case of diabetic acidosis, obviating the use of the 'Acetest' method for estimating the degree of ketonæmia by serial serum dilutions. Because of the sensitivity of the method, blood acetone may be determined when present in concentrations below the sensitivity of the 'Acetest' tablet.

The determination of acetone concentration in the expired air by this method is most promising. It has been possible to detect acetone in the breath of patients with blood concentrations less than 1·5 mgm. per cent, and in the acidotic diabetic it has proved to be the most rapid means of establishing the exact degree of acidosis. As the concentration of air in the alveoli parallels the concentration in the blood, the effect of treatment of the diabetic in acidosis is easily ascertained.

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Influence of Scurvy and Lathyrism (Odoratism) on Hydroxyproline Excretion

Since nearly all the hydroxyproline in the body is found in collagen, it has been suggested[1–3] that abnormal amounts of hydroxyproline may be excreted in conditions which alter the metabolism of collagen. The availability of a specific and relatively simple method for analysing hydroxyproline in urine[4] encouraged us to measure hydroxyproline excretion in scurvy and lathyrism (odoratism), two experimental conditions known to affect collagen.

To study the effect of scurvy, male guinea pigs (Hartley) were fed a scorbutogenic diet[5], and the urinary excretion of hydroxyproline was measured[4] daily for 24 days. Five test animals received the scorbutogenic diet alone, and 5 control animals received the diet and an oral supplement of 1 mgm. of ascorbic acid per day. The hydroxyproline excretion was essentially the same in both groups for the first 19 days on the diet, but from the 20th to the 24th day the animals not receiving ascorbic acid excreted significantly less hydroxyproline (0·45 ± 0·05 mgm. versus 1·12 ± 0·10 mgm. per day). On the 21st day the ascorbic acid-deficient animals showed histological evidence of scurvy[6]. In other experiments guinea pigs were placed on the scorbutogenic diet and divided into three groups : group 1 received 1 mgm. ascorbic acid per day throughout the course of the experiment, group 2 received no ascorbic acid, and group 3 received 10 mgm. of ascorbic acid on the 21st day of the experiment. Urine was collected from the 21st to the 22nd day. As shown in Table 1, urinary hydroxyproline excretions were depressed in animals on the scorbutogenic diet but were rapidly restored to normal by administration of ascorbic acid. On the 22nd day the animals in group 1 weighed 20 per cent less than those in group 2. To demonstrate that the decrease in hydroxyproline excretion was not the result of loss of weight, 15 guinea pigs were starved for 2 days. Their body-weights fell from a mean of 195 gm. to a mean of 168 gm., but their urinary hydroxyproline-levels were unchanged.

Table 1. Decrease of Hydroxyproline Excretion in Scurvy and Increase of Hydroxyproline Excretion in Lathyrism

Animals	Number	Diet*	Supplement	Hydroxyproline per 24 hr. mgm. ± S.E.
Guinea pigs (Group 1)	10	scorbutogenic	1 mgm. ascorbic acid/day	1·13† ± 0·15
(Group 2)	31	,,	none	0·57† ± 0·08
(Group 3)	16	,,	10 mgm. ascorbic acid on day 21	1·61† ± 0·15
Weanling rats	6	low protein	none	0·63‡ ± 0·10
Weanling rats	6	low protein	160 mgm. BAPN§	2·06‡ ± 0·20
Adult rats	6	low protein	none	0·48‡ ± 0·06
Adult rats	6	low protein	160 mgm. BAPN	1·31‡ ± 0·07

* Both diets hydroxyproline-free.
† Hydroxyproline excretions for the twenty-first day of the experiment.
‡ Hydroxyproline excretions for the fifty-fifth day of the experiment.
§ BAPN, β-aminopropionitrile, supplied by Abbott Laboratories.

To study the effect of a lathyrogenic agent on hydroxyproline excretion, weanling rats and adult rats were placed on a low protein diet[7], and half the animals from each group were injected with 160 mgm. β-aminopropionitrile (BAPN) per day. No difference in urinary hydroxyproline excretion between control and injected animals was observed on the 4th and 20th days of treatment, and there was no gross evidence of lathyrism. By the 55th day, the BAPN-treated animals excreted two to three times as much hydroxyproline as controls (Table 1) and all the animals had marked scoliosis. More than 90 per cent of the hydroxyproline in the urine from both control and experimental animals was dialysable.

Several other conditions which might change hydroxyproline excretion were also investigated. Young guinea pigs (11–20 days) were found to excrete significantly more hydroxyproline than adult guinea pigs (2 years old). The young animals, weighing 100–180 gm., excreted an average of 1·84 ± 0·15 mgm. hydroxyproline per 24 hr. as compared with an average of 1·22 ± 0·20 mgm. per 24 hr. excreted by 2 kgm. adult animals. Comparable age differences have been reported previously for humans[1] and rats[8]. Preliminary experiments have confirmed the earlier report[9] that urinary hydroxyproline is not altered by parturition. In addition, our results indicate that urinary hydroxyproline-levels are not altered by parathyroidectomy or the injection of papain or carrageenan.

The results obtained here indicate that the extensive alterations induced in collagen by scurvy and experimental lathyrism, as well as by ageing, are accompanied by significant changes in the amount of hydroxyproline excreted in urine. Although most of body collagen is relatively inert[10], some fractions of collagen are both synthesized and degraded rapidly[8]. While the latter may or may not be identical with the various soluble collagens[11], several observations suggest that the amount of hydroxyproline excreted in urine reflects the amount of body collagen in these metabolically more 'active' forms. Young animals which have more soluble collagen[12] and a more rapid turnover of collagen[10] than adult animals excrete more hydroxyproline. Both the amount of soluble collagen[13] and the synthesis of collagen are decreased in scurvy[14], and the scorbutic guinea pigs excreted less hydroxyproline (Table 1). Lathyrogenic agents increase the amount of soluble collagen[15], and the increased hydroxyproline excretion in the β-aminopropionitrile-treated rats probably indicates an increased amount of metabolically active collagen in lathyrism. It is of interest that experimental lathyrism resembles Marfan's syndrome, and that some patients with Marfan's syndrome excrete increased amounts of hydroxyproline[2]. Measurements of urinary hydroxyproline may be a useful index of collagen metabolism in a number of other experimental and clinical conditions.

We are indebted to Dr. I. Zipkin for his assistance in a phase of this work.

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PHYSIOLOGY

Analogues of Glutamic and γ-Amino-*n*-butyric acids having Potent Actions on Mammalian Neurones

Numerous investigations have shown that certain amino-acids, including several which are present in the mammalian central nervous system, strongly influence the behaviour of neurones when applied extracellularly[1–4]. These amino-acids fall into two groups; neutral amino-acids related to γ-amino-*n*-butyric acid (GABA), which depress the activity of neurones, and acidic amino-acids related to glutamic acid, which excite neurones[3]. An analysis of the effects of these substances on mammalian spinal neurones indicates that whereas the mechanism of action of GABA differs from that of mammalian inhibitory transmitters[5], the action of L-glutamic acid has certain features in common with that of excitatory transmitter substances[3,6]. Recently, several substances were found to have a much greater potency than GABA and glutamic acid as depressants and excitants respectively of neurones within the isolated and sagitally hemisected spinal cord of the toad[7]. In particular, *N*-methylaspartic acid and homocysteic acid were found to be potent excitants, and 3-amino-1-propanesulphonic acid was a powerful depressant. The potencies of these and related amino-acids on mammalian spinal and cortical neurones have now been determined.

Two methods were used. In the experiments on spinal cord, the amino-acids were passed electrophoretically from solutions of appropriate *p*H into the immediate extracellular environment of interneurones located in the lumbar segments of cats anæsthetized with pentobarbital sodium[3]. Multibarrel electrodes were used so that a comparison could be made between the actions of different substances. In these experiments excitation was measured by the frequency of spike discharge evoked by a particular amino-acid and depression by the degree to which synaptically evoked spikes and negative field potentials were reduced in number and magnitude respectively[3]. Relative potencies were assessed on the basis of the electrophoretic currents required to produce a given degree of excitation or depression. The acidic amino-acids were readily passed as anions from electrode solutions adjusted to *p*H 8 with sodium hydroxide. All these compounds were in a similar ionic state at this *p*H and the concentrations of the substances obtained in the extracellular fluid can be assumed to be directly proportional to the electrophoretic currents used[8]. The relative potencies are given in Table 1, *A*.

Difficulty was experienced in passing the neutral amino-acids as anions, presumably because of electro-osmotic influences tending to pass the substances in the opposite direction when the electrodes contain solutions of high *p*H (ref. 3). These amino-acids were therefore passed as cations from solutions made to *p*H 2·8 with hydrochloric acid. Under these conditions less of the sulphonic than of the carboxylic amino-acids are in the cationic form, and the extracellular concentrations attained by the former, using a particular electrophoretic current, were probably considerably lower than those of the carboxylic acids. It is therefore probable that the true potencies of 3-amino-1-propanesulphonic acid and of taurine, relative to GABA and β-alanine, exceed the figures given in Table 1, *C*.

Table 1. Relative Potencies of Amino-acids

Excitants	*A*	*B*	Depressants	*C*	*D*
L-Aspartic acid	3	$> 10^{-2}$ *M*	β-Alanine	3	$7{\cdot}5 \times 10^{-3}$ *M*
D-Aspartic acid	2	$> 10^{-2}$ *M*	Taurine	2–3	$7{\cdot}5 \times 10^{-3}$ *M*
L-Glutamic acid	3	$> 10^{-2}$ *M*	γ-Amino-*n*-butyric acid	3	$2{\cdot}5 \times 10^{-3}$ *M*
D-Glutamic acid	2	$> 10^{-2}$ *M*	3-Amino-1-propane-sulphonic acid	6–15	2×10^{-3} *M*
N-Methyl-L-aspartic acid	2	$7{\cdot}5 \times 10^{-3}$ *M*			
N-Methyl-D-aspartic acid	20–30	$7{\cdot}5 \times 10^{-3}$ *M*			
L-Cysteic acid	3	$> 10^{-2}$ *M*			
D-Cysteic acid	1–3	$> 10^{-2}$ *M*			
L-Homocysteic acid	4–5	not tested			
D-Homocysteic acid	9–18	$2{\cdot}5 \times 10^{-4}$ *M*			
DL-Homocysteic acid	6	5×10^{-4} *M*			

Columns *A* and *C*, approximate relative potencies based on the responses of spinal interneurones to electrophoretically applied amino-acids. Column *B*, minimum concentrations of the amino-acids in Ringer solution required to produce spreading depression within exposed parietal cortex; topical application. Column *D*, minimum concentrations of the amino-acids in Ringer solution required to abolish negative phases of positive-negative potential evoked in parietal cortex by transcallosal stimulation; topical application.

In the cortical experiments, the amino-acids were applied as serially diluted solutions in buffered mammalian Ringer solution (*p*H 7·0–7·2) to the exposed parietal cortex of cats anæsthetized with pentobarbital sodium, and electrical records were obtained from the same cortical surface. The equivalent concentrations of depressants listed in Table 1, *D* are the average of those which just abolished the negative phase of the positive–negative potential sequence evoked by transcallosal stimulation[9]. The equivalent concentrations of excitants (Table 1, *B*) are the average lowest concentrations which produced spreading depression within two minutes of application, this phenomenon being indicated by the abolition of transcallosally evoked responses and the recording of a surface negative potential of 6–12 mV.[2,10]. A pore type of electrode permitted the recording of cortical potentials while the solutions were being applied[10].

Although many factors may limit the quantitative reliability of each of these methods, there is nevertheless a clear qualitative correlation between the two sets of results. It is apparent that 3-amino-1-propanesulphonic acid is considerably stronger than GABA, β-alanine or taurine, which were previously considered to be among the most potent of the amino-acids having depressant actions on mammalian spinal neurones[3]. 3-Amino-1-propanesulphonic acid bears the same structural relationship to homocysteic acid as GABA bears to glutamic acid, and the strong excitatory action of homocysteic acid was therefore to be expected[3]. The D-form of this compound is stronger than the L-form. A similar, though very much larger, difference is evident between the two enantiomers of *N*-methylaspartic acid, the D-isomer in this case being the most potent amino-acid excitant yet tested. The potency of *N*-methyl-D-aspartic acid is more remarkable when it is considered that alkyla-

tion of members of both the excitant and depressant series of amino-acids has usually led to lower activity[5,7]. The significance of these results in relation to the general mechanism of amino-acid excitation and depression, including the possible participation of membrane phospholipids in such processes, will be discussed elsewhere.

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Inhibition by Potassium of the Mechanical Responses of Frog Muscles at Low Temperature

Csapo and Wilkie[1] reported a curious effect of temperature on the twitch tension in the frog's sartorius muscles, as influenced by the action of 24 m*M* potassium chloride. The onset of the fall of tension development and the depolarization were delayed by lowering the temperature from 23° to 2° C., corresponding to a Q_{10} of 1·2–1·3. On the other hand, the rate of recovery, when normal Ringer solution was replaced, was very much reduced at lower temperature. However, if the recovering muscle were suddenly warmed again, the twitch tension recovered immediately. Grieve[2] has shown that, when recovery was delayed, it was due to delayed re-polarization. If chloride were replaced by the impermeant methyl sulphate, then there was no delay in re-polarization that could not be accounted for by diffusion in the interspaces.

I have studied the inhibition by potassium of isotonic twitching and contractions provoked in the sartorius and rectus muscles. The methods have been described[3]. The muscle was placed into a double-wall muscle chamber, cooled in its outer compartment by a fluid at −2° C.; the solution inside the chamber decreased in temperature from 20° to 0° C. within 2 min.

Both the sartorius and the rectus muscle were unable to give twitch or phasic contracture-like responses comparable in height to the control muscles, after application of potassium chloride or potassium sulphate lasting 1 min. or more, unless the time of recovery was 5–10 min. After 40 sec. of recovery, only 23 per cent of the control height of the contractions was obtained in the sartorius, when 115 m*M* potassium chloride or 95 m*M* potassium sulphate were applied. 4 per cent of the twitch response when 20 m*M* potassium chloride was applied and 15 per cent of the rectus first phase response[3] when 6–10 m*M* potassium chloride was applied. After the same solutions were allowed to act for the same intervals at low temperature, and the subsequent time of recovery of 40 sec. occurred at room temperature, the corresponding responses were several times larger, the percentage values being 63, 69 and 70. Responses of the same magnitude as the percentages mentioned were obtained if the action of potassium chloride or potassium sulphate at the lower temperature was prolonged in the sartorius about three times (contractures) and in the rectus for more than five times (Fig. 1).

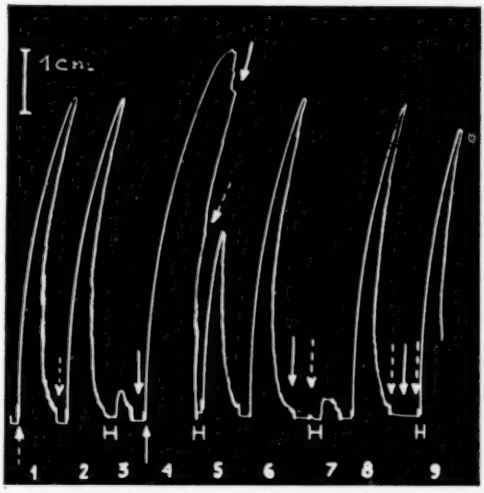

Fig. 1. Frog's rectus. Phasic response to 10 m*M* potassium chloride; 0·5 m*M* calcium in Ringer solution. Arrows upward, application of potassium; arrows downward, washing; full arrows, cold solutions (0° C.); interrupted arrows, solutions at room temperature (20–23° C.). *H*, Short interval (40 sec.); 1, 2, 3, control reactions; 4, 5, reaction at low temperature and, a short time after it, at room temperature; 6, after a reaction at room temperature, washing (15 min.) at low temperature, 40 sec. before 7, at room temperature; 8, after the reaction, first a 10-min. wash at room temperature, then at low temperature, 40 sec. before 9, again at room temperature

Low temperature, if applied a few seconds after the exposure to potassium had been terminated, prolonged the inhibition by potassium: no significant recovery could be obtained at 0° C. for 20 min. of washing. The same results were obtained with potassium chloride as well as with potassium sulphate. No rapid recovery within 40 sec. of warm washing, after prolonged washing in a cold Ringer solution, was observed.

The inhibiting action of potassium on mechanical responses at low temperature in the present experiments seems to follow a course which is slowed down relatively more than that of the decrease in twitch tension. The absence of a rapid recovery during the short period, after warming up, and the lack of differences in the rate of recovery after application of potassium chloride and potassium sulphate, prove that the two processes may indeed be different.

In a further group of experiments the response of the sartorius muscle to potassium was examined, after prolonged washing (20 min.) in a cold calcium-free solution. The effect was the same as that at room temperature, that is, the contraction was inhibited[4]. If the muscle were washed first in a calcium-free Ringer solution at room temperature, then cooled and washed in a cool, Ringer solution containing calcium, it regained the ability to respond to potassium by a contraction.

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Fatigue in a Fluid-filled Pulsating Elastic Tube

Experiments with the 'oscillator' described by me[1] have shown that fatigue affects the elastic tube, after it has been pulsating for several hours.

Aneurysmal-like bulges may occur. Alternatively, a small hole forms at the end close to the rigid outlet tube, presumably due to cavitation occurring in contact with the wall. Fluid may escape from this hole, or air be sucked in, at each pulse.

A remarkable effect is obtained by the application of cedar wood oil. Within a few minutes, the distal segment of the elastic tube, when so treated, becomes permanently stretched into a sigmoid shape and develops periodic constrictions and dilatations, affecting the system of short wave-length, that is in the range 1–2 cm.

These figures resemble the shapes of arteries in atherosclerosis.

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PHARMACOLOGY

Alleged Anticomplementary Effect of Aspirin

Van Oss *et al.*[1] recently reported that aspirin at concentrations of $2{\cdot}2 \times 10^{-3}$ *M* (40 mgm./100 ml.) inhibited the lysis of red cells in a simple complement-hæmolysis reaction, but that sodium salicylate was inactive. This was interesting in view of the therapeutic action of aspirin against diseases such as the rheumatic disorders which are thought to involve immunological phenomena[2]. Two other related compounds, salicylaldoxime and salicylaldehyde, were earlier reported[3] to inhibit red cell lysis in a similar reaction. I therefore investigated the complement-hæmolysis technique since it might usefully predict anti-rheumatic activity in other salicylate derivatives.

Sheep erythrocytes (*E*), anti-sheep erythrocyte serum prepared in the rabbit (*A*) and complement (*C′*) were standard serological reagents obtained from the Commonwealth Serum Laboratories, Melbourne. Complement was received as ampoules of freeze-dried guinea pig serum which was reconstituted with distilled water and stored at 4° C. It gave results identical with fresh guinea pig serum prepared in these laboratories. Barbital buffer, *p*H 7·4 and containing added Mg^{++} and Ca^{++}, was prepared according to Mayer *et al.*[4] and diluted 1 in 5 with distilled water on the day of use. All reagents were diluted in barbital buffer. When 0·1 ml. of a 2 per cent suspension of washed erythrocytes was sensitized with 0·1 ml. of antiserum 1/1,000, the addition of 0·5 ml. of guinea pig serum 1/200 caused complete hæmolysis of the erythrocytes within 30 min. at 37° C. The effect of added substances on the progress of the *C′* hæmolysis was determined by placing 0·25 ml. of guinea pig serum 1/100 into hæmolysis tubes and adding 0·25 ml. of a solution of the compound in barbital buffer. The latter solution was replaced by buffer alone in control tubes. The diluted antiserum and red blood cells were then added and the tubes incubated at 37° C. for 30 min. The *p*H of the solutions prior to incubation was determined with a glass electrode in duplicate tubes, which were prepared for each concentration of the compound under test.

The substances I have investigated and the concentrations that caused an approximate 50 per cent inhibition of hæmolysis in our procedure are shown in Table 1. All the acids decreased the *p*H of the final mixture proportional to their concentration, and the *p*H range at the concentrations where hæmolysis was inhibited are shown in column 2. Where applicable I have included the inhibitory concentrations reported by other workers for comparison.

Table 1. Substances which inhibited Complement-Hæmolysis and their Effect on the *p*H of the Reaction Mixture

Compound	Concentrations (molar) giving 50 per cent inhibition of hæmolysis in 30 min.	*p*H ranges of the reaction mixtures at the inhibitory concentrations	Concentrations (molar) elsewhere reported to inhibit complement-hæmolysis	Ref. No.
Aspirin	$2{\cdot}3 \times 10^{-3}$	5·8–6·4	$2{\cdot}2 \times 10^{-3}$	1
Salicylic acid	$2{\cdot}3 \times 10^{-3}$	5·8–6·4	10 per cent inhibition $3{\cdot}3 \times 10^{-3}$	
Sodium salicylate	No inhibition	7·4	No inhibition	1
Fumaric acid	$2{\cdot}3 \times 10^{-3}$	5·8–6·2	—	—
Hydrochloric acid	$2{\cdot}3 \times 10^{-3}$	5·8–6·4	—	—
Salicylaldoxime	8×10^{-4}	7·5	5×10^{-4}	3
Salicylaldehyde	8×10^{-4}–10^{-3}	7·3	$2{\cdot}5 \times 10^{-4}$	3

Several important points emerge from Table 1. The capacity of the diluted Mayer solution was clearly inadequate to buffer the effects of the acidic compounds on the *p*H of the hæmolytic reaction. Furthermore, it seemed strange that while salicylic acid inhibited hæmolysis, sodium salicylate at similar concentration was inactive—yet at *p*H values above 5, both compounds would be in solution in identical form as salicylate ions. Finally all the acids tested inhibited my hæmolytic reaction at approximately the same molar concentration ($2{-}3 \times 10^{-3}$), hydrochloric acid being just as effective as the organic acids. At this concentration they reduced the *p*H of the reaction from 7·4 to about 6. It appeared likely therefore that inhibition of hæmolysis was due in some manner to the *p*H shift rather than a specific anticomplementary action. This explanation was further justified when the above experiments were repeated using undiluted Mayer buffer, the tonicity of which was adjusted by omitting some of the sodium chloride. Under these conditions hæmolysis was complete within 30 min. in the absence of a test compound and was inhibited by salicylaldehyde and salicylaldoxime at the same concentrations as in the dilute buffer. However, none of the other substances in Table 1 lowered the *p*H of the final mixture below 7·0 or caused any inhibition of complement-induced

hæmolysis at concentrations below 5×10^{-3} *M*. Subsequent investigation with buffers of even greater capacity for hydrogen ions failed to show an anticomplementary effect of the acids in the absence of *p*H shifts at concentrations up to 10^{-2} *M*.

In conclusion, my results suggest that aspirin has no real effect *per se* on complement-induced hæmolysis of red blood cells. Since it is a weak acid, it may, if it is insufficiently buffered, lower the *p*H of a complement-hæmolysis reaction to the point where there is an unspecific inhibition of hæmolysis. The ability to do this is probably common to all acids.

It is of some interest that the anticomplementary effect of aspirin has been reported to occur also *in vivo*[1]. However, since no details were given, no comment is possible.

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HISTOLOGY

Neurosecretion and Granules in Neurones of the Brain of the Leech

Examination of established neurosecretory structures with the electron microscope reveals characteristic cytoplasmic structures: the elementary neurosecretory granules. These granules are usually electron-dense and range in diameter from about 1000 Å. to about 3000 Å. Mean granule diameters differ from cell to cell in the same and in different species; but the fully formed granules are of relatively constant size for each cell type.

The elementary neurosecretory granule has been found in a variety of neurohæmal organs in representatives of many animal groups: the pars nervosa of vertebrates (mammal[1], bird[2], reptile[3], amphibian[4], and teleost[5]), the corpus cardiacum of insects[6], the sinus gland[7] and the post-commissural organ[8] of crustaceans. More recently, similar granules have been observed in the perikarya of neurosecretory cells of the caudal neurosecretory system of teleosts[9], of the preoptic nucleus of teleosts and amphibians[10,11], of the pars intercerebralis of insects[11,12], and of the brain of annelids[11,13]. The ubiquity of these elementary granules in neurosecretory systems has led to the conclusion that their presence may be virtually diagnostic of neurosecretion at the ultrastructural level.

Examination of the brain of the leech, *Theromyzon rude*, with the electron microscope reveals that every neurone contains at least some granules in the size range of elementary neurosecretory granules. (Electron microscope observations were made of the supra-œsophageal ganglion of *Theromyzon rude* fixed in Palade fixative with sucrose, embedded in methacrylate, sectioned on a Porter-Blum microtome, and observed with an R.C.A. *EMU*-3 electron microscope.) However, with the light microscope three principal types of neurones can be distinguished[14]. Two of these cell types contain cytoplasmic inclusions suggestive of intense glandular activity; cells of the third type, comprising the vast majority

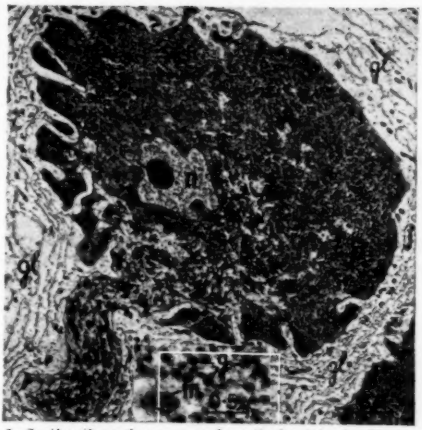

Fig. 1. Section through a neurone from the leech brain. Numerous dark neurosecretory granules are present in the cytoplasm and axone. *n*, Nucleus with a nucleolus; *a*, axon; *gl*, glia cell cytoplasm; *p*, polystyrene particle (2640 Å.). Insert. Portion of cytoplasm from a similar type of cell. *g*, Neurosecretory granules; *m*, mitochondrion

(95 per cent) of the neurones of the brain, show signs of minimum glandular activity only. Axons from the secretory-appearing cells pass into the dorsal commissure and appear to terminate in a neurohæmal area at its posterior surface. With the electron microscope at least some of the glandular-appearing neurones contain enormous numbers of elementary neurosecretory granules, both in the cell body and in the axon (Fig. 1). In the presumed neurohæmal area, the elementary neurosecretory granules become

Fig. 2. Frontal section of the posterior portion of the dorsal commissure. Shown are axon endings of presumed neurohæmal structure. *g*, Neurosecretory granules; *sv*, synaptic vesicles; *pn*, perineurium

intermixed in the axones with small vesicles morphologically equivalent to synaptic vesicles (Fig. 2). Such vesicles have been reported in other neurohæmal structures[1,2,4].

On the basis of ultrastructural cytological criteria, including the consistent association of elementary granules with Golgi membranes[11], all neurones in the brain of the leech would have to be defined as neurosecretory. Since the majority of neurones in the brain must be involved in 'ordinary' nervous activities, one is hesitant to ascribe neurosecretory activity to all of them. It is possible that some of the types of granules in the leech neurones may be associated with neurotransmitter function. The usual synaptic vesicle is small (< 500 Å.)[15] and relatively electron-lucent. However, our knowledge of the range of size and appearance of presumed neurotransmitter granules is limited, particularly with respect to non-cholinergic neurones. Conceivably many of the larger, more electron-dense granules seen in leech neurones could be synaptic granules of a type associated with non-cholinergic neurohumours. Only those granule-bearing cells terminating in the neurohæmal area would be considered as properly neurosecretory.

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PATHOLOGY

Stimulation by Heparin of Parenchymal Liver Cell Proliferation in Normal Adult Rats

It has been reported[1] that serum from hepatectomized rats when injected into normal rats will stimulate mitosis in the livers of the recipients. This finding could not, however, be confirmed by other workers[2]. In work reported earlier[3], we found that plasma from hepatectomized rats, which was collected using heparin to prevent clotting, stimulated both mitosis and deoxyribonucleic acid (DNA) synthesis in rat liver parenchymal cells, when injected into normal rats. On the other hand, serum or citrated plasma from hepatectomized rats had no apparent stimulating effect. For this reason, the effect of heparin itself on the proliferation of normal rat liver was examined.

Groups of five male Holtzman rats, 250–280 gm. body-weight, were used in each experiment. In a typical experiment, 100 units of heparin in 3 ml. of physiological saline or saline alone was injected intraperitoneally. If mitotic activity in the liver was to be examined, the animals were killed 24 hr. after the sample injection. Paraffin sections were cut at 6μ and stained with Harris's hæmatoxylin. Mitotic activity in parenchymal liver cells was expressed as mitotic figures in 50 high-power fields (× 400).

The effect of heparin on DNA synthesis in the liver was determined by intraperitoneal injection of orotic acid-6-^{14}C (2·5 μc./rat) 7 hr. after sample injection. The animals were killed 17 hr. later and DNA isolated essentially by the method of Hecht and Potter[4]. Isotope measurements were made using a Nuclear Chicago *D*47 gas-flow counter equipped with a 'Micromil' window. Specific activity is expressed as counts/min. per mgm. DNA.

Table 1. Effect of Heparin and Other Sulphated Polymers on DNA Synthesis and Mitotic Activity in Normal Adult Rat Liver

Sample injected	Mean mitotic index		Mean specific activity (c.p.m./mgm. DNA)		
	Exp. I	Exp. II	Exp. III	Exp. IV	Exp. V
Saline control	6 (1–11)*	6 (1–11)	15 (8–20)	18 (10–23)	9 (4–16)
Heparin (100 units)	62 (26–113)	74 (27–113)	68 (27–113)	92 (45–162)	64 (30–136)
Heparitin sulphate	—	—	—	—	115 (30–208)
Chondroitin sulphate *B*	—	—	—	—	34 (16–60)
Chondroitin sulphate *A*	—	—	—	—	19 (12–26)
Chitin sulphate	—	—	—	—	15 (9–22)
Polystyrene sulphonate	—	—	—	—	5 (4–7)

* Range in parentheses.

The results obtained from representative experiments using heparin are shown in Table 1. It is apparent that intraperitoneal injection of heparin resulted in marked stimulation of mitotic activity in normal rat liver parenchymal cells. The increase in mitotic activity is accompanied by increased incorporation of the radioactive precursor into DNA. Microscopic examination showed no apparent alteration in liver structure under these conditions.

To determine whether the response of the liver was specific for heparin, a number of natural and synthetic sulphated polymers were also examined for their ability to stimulate DNA synthesis in the liver (Table 1). One mgm. of each polymer in physiological saline was administered under the same conditions described here. Heparitin sulphate, which differs in structure from heparin only slightly[5], having some of the *N*-sulphate groups replaced by *N*-acetyl, stimulated liver DNA synthesis to the same extent as heparin. The higher value of the single experiment cited is not significant. Chondroitin sulphate *B* (β-heparin) also had some activity in this system, whereas, chondroitin sulphate *A*, chitin sulphate and polystyrene sulphonate had little or no stimulating effect. There was no change in this pattern on increasing the dose to the 5-mgm. level. The ability of the various polyanions to stimulate liver DNA synthesis appears to parallel the anti-coagulant and lipæmia-clearing activity of these substances.

The marked stimulation of liver-cell proliferation by heparin at doses above 20 units/rat suggests that

the report[3] of stimulatory activity in hepatectomized rat plasma reflected the presence of heparin in the plasma.

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Use of Desertomycin Medium in Routine Selective Isolation of Fungi Pathogenic to Man

The isolation of pathogenic fungi producing superficial or systemic diseases in man from clinical materials is made difficult by different contaminating bacteria and saprophytic fungi overgrowing the culture medium and suppressing or inhibiting the growth of pathogenic fungi. In order to obtain pathogenic fungi in a pure culture, attempts were made to mix various substances in the medium in such a way that they would be liable to prevent the growth of contaminants. After a great deal of effort, the most satisfactory result was obtained by cultivation media containing actidione. Actidione (cycloheximide)[2] was the only antifungal antibiotic which, though exerting no antibacterial effect, inhibits the growth of most saprophytic fungi, yet does not inhibit the development of fungi pathogenic to man. For this very reason, it can be used with penicillin and streptomycin[2,3] or chloramphenicol[4] for the preparation of selective culture media.

The antibiotic isolated from *Streptomyces* and a completely new type called desertomycin, which is produced by us in a crystalline form, reveals a selective anti-fungal effect similar to actidione. Previously we reported on its anti-bacterial and cytostatic effect *in vitro*[5,6]. Our recent investigations, however, disclosed also its anti-fungal effect *in vitro*. Its anti-fungal effect on saprophytic fungi is much stronger than on fungi pathogenic to man. The anti-fungal concentration inhibitory to various fungi of desertomycin incorporated in Sabouraud–dextrose–agar was as follows: (*A*) *Streptomyces* strains: 1–10 μgm./ml. (*B*) Most of phytopathogenic and saprophytic filamentous fungi: 10–25 μgm./ml. (*C*) Dermatophytes: 50–100 μgm./ml. (*D*) Yeasts and yeast-like fungi (except *C. neoformans*): 250–500 μgm./ml.

By dint of its previously disclosed anti-bacterial property and the selective anti-fungal effect revealed by the findings, desertomycin promised to be a suitable agent for preparing culture media on which fungi pathogenic to man can be cultivated in pure culture.

These investigations were completed with cultivations of clinical materials liable to produce fungal contamination (such as hair, skin-scrapings, nail-clippings, sputum, etc.). The clinical materials were inoculated partly on Sabouraud-agar containing no antibiotics and partly on Sabouraud-agar containing desertomycin of 30 and 50 μgm./ml. The tubes were usually incubated for three weeks at 27° C., and the cultures were read. The results are summarized in Table 1.

Table 1

Total No. of materials	Total No. of isolations	Positive on both agars	Positive on desertomycin agar, negative on Sabouraud-agar
308	77	24	53

77 isolates were obtained from a total of 308 substances. 24 cultures of dermatophytes were obtained on both Sabouraud- and desertomycin-agars. 53 cultures of fungi pathogenic to man were isolated on desertomycin media but not on the corresponding tubes of Sabouraud-agar. These results reveal the superiority of desertomycin-containing medium to Sabouraud-agar as a routine isolation medium for fungi pathogenic to man from clinical materials.

Although desertomycin differs altogether from actidione in its chemical, physical and some biological properties, the two antibiotics closely resemble one another in their selective anti-fungal characteristics. Both can be used for the selective isolation of fungi pathogenic to man. The disadvantage of desertomycin as against actidione is that its quantities inhibitory to the growth of pathogenic and apathogenic fungi do not differ so much. On the other hand, desertomycin possesses several advantageous properties as against actidione, such as (1) stability; resistance to heat; it can be autoclaved with the culture medium without loss of activity; (2) it has also an anti-bacterial effect and so does not require the addition of any anti-bacterial antibiotic; (3) about one-tenth of the concentration is needed to attain the same effect as in actidione; (4) it does not irritate or damage the skin.

As a result of these findings, the culture medium containing desertomycin provides new possibilities for dermato-mycological laboratories. Detailed description of the experiments will be reported elsewhere.

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RADIOBIOLOGY

Radiation Sensitivity of *Escherichia coli* to β-Rays in Dry and Wet Condition

The greater sensitivity of biological material to irradiation in the wet condition is due to the formation of radicals in the water surrounding the vital molecule. However, all the volume is not effective, since these radicals have a definite life-time and can diffuse only to a distance allowed by that time. This diffusion distance has variously been estimated from different types of inactivation studies in the wet and in the dry state of the molecules. Recently, the dependence of this distance on the shape of the molecule has been reported, and it has been shown that as the asymmetry of the target increases, the

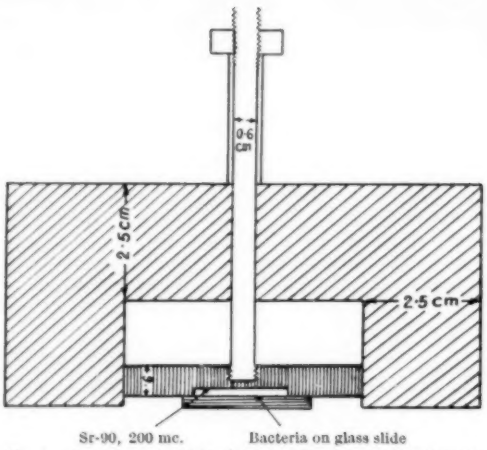

Fig. 1. Arrangement of β-irradiation of *E. coli* from strontium-90 source

diffusion distance decreases[1]. In the present report this idea has been applied to the calculation of diffusion distance for the inhibition of colony-formation by *E. coli* exposed to β-rays from strontium-90.

E. coli was grown in peptone water (peptone 1 per cent, sodium chloride 0·5 per cent in distilled water) for 4 hr. at 37° C. At the end of this period, bacteria were freed from the culture medium by repeated centrifugation and washing with Sørensen's buffer ($M/15$ KH_2PO_4 and $M/15$ Na_2HPO_4 3 : 7 v/v) at *p*H 7·00. This cell suspension, freed from broth medium, was used in different experiments.

The source of β-irradiation (obtained from the Radiochemical Centre, Amersham, England) was a strontium-90 plaque 2 cm. × 2 cm., as shown in Fig. 1. It was kept in a lead-lined 'Plexi-glas' box, the distance between the source and the sample being controlled by an adjustable screw. The total distance between the source and the sample was 2 mm.; the dose at this distance measured with an extrapolation ionization chamber was 2,460 ± 368 rads/min.

For irradiation in the wet condition, glass slides were covered with thin films of gelatin and one drop of mother suspension was taken on the slide. These glass slides containing the bacteria could be placed under the β-source in the specified position shown in Fig. 1, the centre of the drop being accurately in line with the centre of the sources. For irradiation in the dry condition, the gelatin covered glass slides containing bacteria were first dried in a vacuum desiccator for 48 hr. The preliminary tests for desiccation and the conditions of viability of the bacteria after the desiccation process have been described in detail in a previous paper[2].

Slides prepared in exactly identical ways, but not subjected to irradiation, were kept as controls. Agar platings were done by releasing bacteria both from the control and the irradiated slides separately in known quantities of phosphate buffer. Visible colonies were counted after 24 hr. of incubation at 37° C. in order to obtain the viable count in the control and irradiated samples. The ratio N/N_0 could thus be obtained for each dose of radiation, both in the wet and in the dry condition of irradiation.

Fig. 2 shows the results of irradiation of *E. coli* in dry and wet condition, with β-rays from strontium-90. 37 per cent survival doses in dry condition is 20,000 rads and that in the wet condition is 6,000 rads. If the inactivation is due to a single-hit type of process and there are n equally sensitive targets per cell the survival ratio $S = N/N_0$ after dose of D rads is given by:

$$S = \exp - [I\sigma n V] \qquad (1)$$

where $I = 5{\cdot}68 \times 10^{11} \times D$ primary ions per c.c. of unit density material, σ, the density of the material and V, the volume of the target responsible for inactivation. From the relation (1) the target volumes are found to be $6{\cdot}6 \times 10^7$ Å.^{3} and $29{\cdot}3 \times 10^7$ Å.^{3} for dry and wet conditions respectively. The density of the target material in the dry and wet condition has been assumed to be 1·33 and 1·00 respectively. Additional sensitive volume ΔV in the hydrated condition is thus $22{\cdot}7 \times 10^7$ Å.^{3}. For n equally radiation-sensitive targets per cell the additional sensitive volume is given by:

$$\Delta V = nYqv \qquad (2)$$

where Y is the number of radicals formed per primary ionization in water, q the probability of reaction between the radicals and the target and v is the effective volume of water from which the radicals can come and react with the target.

The mechanism of the inhibition of division by radiation is not clear. Very possibly it is a gene-controlled process, so that the action of radiation on any one of the sub-units of DNA molecules located in the chromosomes might be responsible for the inhibition of division[3,4]. A recent estimate of the genetic unit from the transforming principle activity is 20 Å. in diameter and 3400 Å. in length[5], giving a volume $V = 10^6$ Å.^{3}. This leads to a number $n = 60$ from the experimentally observed total volume $nV = 6{\cdot}6 \times 10^7$ Å.^{3}. For the inactivation of DNA, the value of effective radicals formed per primary ionization Yq is 1·5 (refs. 6 and 7).

The volume of available water per target v is obtained from equation (2) as $2{\cdot}5 \times 10^6$ Å.^{3}. Con-

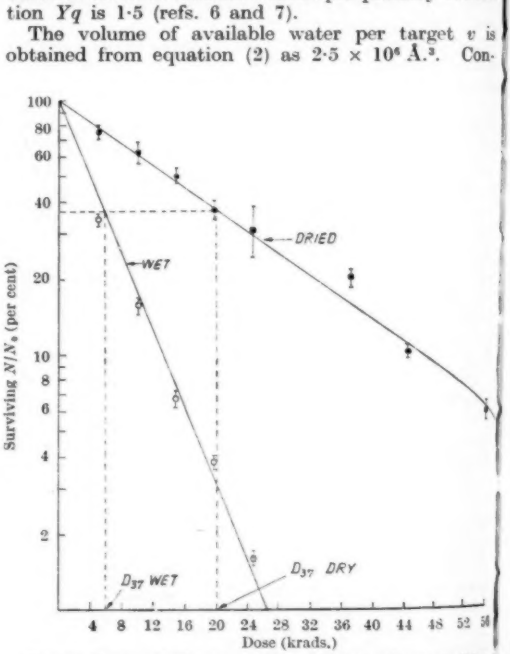

Fig. 2. Dose-survival curves of *E. coli* in dry and wet condition exposed to strontium-90 β-particles

sidering the maximum value of hydration as 40 per cent for protein, the hydration volume per target is $0{\cdot}44 \times 10^6$ Å.^{3}. The excess water $2{\cdot}1 \times 10^6$ Å.^{3} per target must be outside the target and not inside. Assuming the distribution of this water in a uniform layer around the target, the thickness of water surrounding each of the hydrated targets comes out to be 6·5 Å. So besides the hydrated target the radicals can come only from 2 or 3 molecules thickness of water.

This work was carried out under the financial sponsorship of the Council of Scientific and Industrial Research. One of us (S. B. B.) is indebted to it for a senior research fellowship.

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Gross Beta-Activity in the Thames River (Conn.)

Gross β-radioactivity has been determined in a number of marine organisms in the Thames River, Connecticut, United States. Collection areas were selected on the basis of established beds. Samples have been obtained and assayed every two weeks since April 1960. Organisms were returned to the Laboratory immediately after collection, washed free from surface contamination, weighed wet, dried under infra-red and ashed. Aliquots of the ash were prepared for radioassay and counted in an automatic β-counting system.

Radioassays were conducted on the following organisms: *Crassostrea virginica* (oyster), *Mercenaria mercenaria* (clam), *Mytilus edulus* (mussel), *Modiolus demissus* (mussel), *Laminaria agardhii*, *Fucus vesiculosus*. *Ulva lactuca* and *Chondrus crispus* (algæ). The β-activities obtained from these organisms exhibited seasonal variations, with *Laminaria* showing highest values and the oyster the lowest values.

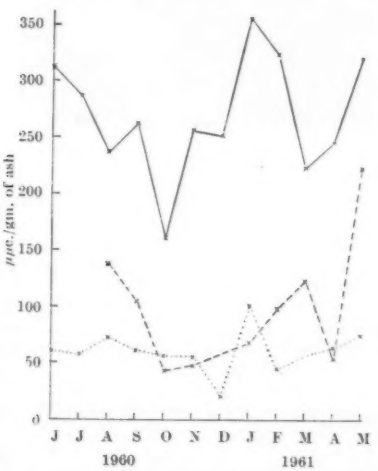

Fig. 1. Radioactivity in organisms obtained from the Thames River (Connecticut). —, *Laminaria agardhii*; · · ·, *Crassostrea virginica*; – –, *Mercenaria mercenaria*

Average monthly radioactive values for three of the above organisms are recorded in Fig. 1. Locations from which these organisms were obtained are: *Laminaria* from a point north of the Electric Boat Co., *Mercenaria* from a bed opposite the United States submarine base and *Crassostrea* from a bed north of the submarine base.

Maximum levels of radioactivity occurred in January 1961, in *Laminaria* with a total of 356 μμc./gm. of ash. Minimum values recorded during the year were in oysters in December 1960, with 23 μμc./gm of ash.

On the basis of analyses for total potassium in these organisms and calculations as to the amount of radioactivity contributed by natural potassium-40 it can be determined that most of the β-activity observed is due to potassium-40. It can be concluded, therefore, that the quantity of radioactivity present in marine organisms studied is extremely small, and does not represent a health hazard.

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BIOLOGY

Biological Incorporation of Implant Material in the Rat

When plastic material is used as a biological implant, to improve contour or function, one of four things commonly occurs: it may cause an acute inflammatory episode and be rejected; rejection may be delayed for several months or years; it may be removed piecemeal by the body's own phagocytes; it may become encysted by fibrous tissue and remain *in situ*. In the last situation the implant has become virtually extracorporeal.

During a recent investigation of various plastic materials in rats, a completely new biological reaction, that of incorporation, was noted. The method of investigation was as follows: through a midline incision the abdomen was opened and a square 2 × 2 cm. of peritoneum and muscle was excised from the lateral abdominal wall. The defect created was covered by the material under test and sutured in place. The abdomen was then closed (Fig. 1). Naked-eye and microscopic examination of the implants were made at various times up to three months later.

In six animals a sheet of polytetrafluoroethylene, 0·3 mm. thick, was used. This material appeared to cause very little tissue reaction, and in four rats it became completely encapsulated by fibrous tissue. But in two animals normal peritoneum grew over the material. The histological picture in these two rats was quite unique; the polytetrafluoroethylene sheet

had become incorporated within the tissues, with no abnormal cellular reaction. In Fig. 2, normal endothelial cells of peritoneum (*A*) are seen covering the polytetrafluoroethylene (*B*), which is permeated by normal-looking blood vessels and occasional tissue cells. The fibrous nature of polytetrafluoroethylene is evident and on superficial examination gives the appearance of collagen fibres. Deep to the implant is the normal muscle (*C*) of the abdominal wall: at this site there is no fibrous capsule nor cellular reaction to the plastic material. Examination under higher magnification (Fig. 3) confirmed the normal appearance of the blood vessels and cells within the polytetrafluoroethylene, suggesting that this was in fact a 'living implant'.

Measurements made from the histological sections indicated that the polytetrafluoroethylene sheet had increased in thickness by 30 per cent, and moreover it is stained faintly by basophilic dyes whereas before implantation it was not possible to stain this material with any of the routine histology stains. A piece of the original polytetrafluoroethylene sheet was subjected to analysis by Mr. G. W. Bowley, of Imperial Chemical Industries, Ltd., and found to be of good quality made from granular polymer, and there was no change in thickness on heating to relieve internal stresses. One is therefore driven to the conclusion that living matter is able to invade the discontinuous structure of a polytetrafluoroethylene moulding and, in so doing, open up this structure to make it readily porous to water. The effects observed in the biological alteration of the implant are thus due to physical and not chemical causes. Polytetrafluoroethylene, unlike other thermoplastics,

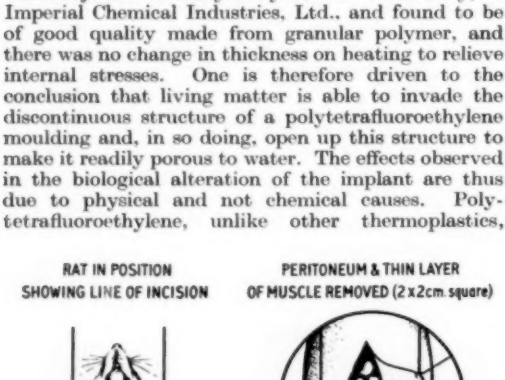
RAT IN POSITION
SHOWING LINE OF INCISION
PERITONEUM & THIN LAYER
OF MUSCLE REMOVED (2 x 2 cm. square)
IMPLANT SEWN INTO DEFECT
REPLACING PERITONEUM

Fig. 1. Diagram of implant techniques

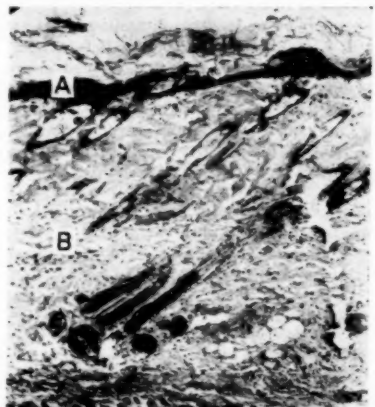

Fig. 2

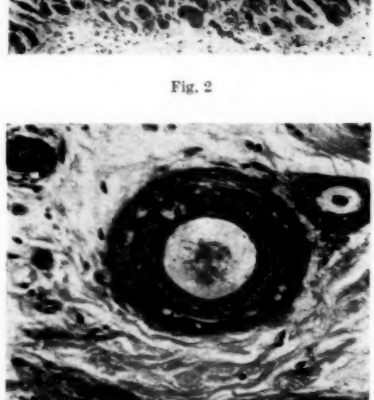

Fig. 3

does not have any melt state and has to be processed by methods similar to those used in powder metallurgy. Polytetrafluoroethylene fabrications are thus prone to void inclusions. No explanation, however, can be offered for the elongation of the polymer granules into fibres although elongation of particles does occur when the polymer in the dispersion form is processed into tape by extrusion and calendering: but the sheets of polytetrafluoroethylene used as implants has not been processed in this way.

These findings open up new possibilities for implant surgery.

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Rapid Development of Hypomagnesæmia in Lactating Cows given Artificial Rations Low in Magnesium

THE most characteristic biochemical change associated with the disorder of cattle known variously as grass staggers, lactation or nutrition tetany and Hereford disease is a fall in the blood-serum magnesium concentration from a normal value within the range 2·0–3·5 mgm./100 ml. to one of 0·7 mgm. or less. The disorder has its highest incidence in milking cows in spring, shortly after a change from winter rations to the grazing of pasture, and under such conditions frequently the fall in serum magnesium concentration is rapid; in extreme cases serum magnesium values of less than 0·7 mgm./100 ml. are observed within two days of the change of diet.

We[1,2] have previously demonstrated with milking cows that a change from winter rations to grazing is commonly associated with a decrease in the net uptake of magnesium from the gut (magnesium in the feed less the magnesium excreted in the fæces), due to a reduction in the dietary intake of magnesium and, to a lesser extent, to a decrease in its availability to the animal: hypomagnesæmia occurs in animals in which the net uptake of magnesium from the gut is reduced to such an extent that it fails to meet the magnesium requirements of the animal for milk secretion, body retention and fœtal growth, and in which, as a consequence, there is a drain on the small amount (2–3 gm.) of freely available magnesium in the blood serum and other extra-cellular fluids. The rapidity of development of hypomagnesæmia implies that the milking cow, during periods of acute magnesium deficiency, has little ability to mobilize any of the considerable amounts (100–200 gm.) of magnesium present in her soft tissues and bones.

Following the early investigations of Blaxter *et al.*[3], I have made a number of attempts to develop for milking cows artificial diets low in magnesium with the object of determining the effect of a rapid reduction in dietary magnesium on serum magnesium concentration, and of ascertaining the minimum amount of dietary magnesium, in a suitable form, on which an animal can maintain normal serum magnesium concentrations. In such work, two main problems have to be overcome: the technical difficulties of preparing the necessarily large quantities of foodstuffs sufficiently low in magnesium, and the need to ensure that such foodstuffs are acceptable to the milking cow and do not lead to digestive disturbances and subsequent loss of appetite and milk yield. It seemed preferable in the first instance, therefore, to develop rations based largely on feedingstuffs in common use, but early experiments showed that the inclusion of large amounts of processed feeds low in magnesium, such as groats or flaked maize, effectively reduced magnesium intake but also improved markedly the assimilation of the magnesium present in the roughage part of the ration. An artificial roughage low in magnesium was therefore prepared from paper pulp[4]. Daily rations of wheat straw (2 lb.), paper pulp treated with liquid invert sugar (6 lb.), flaked maize (14 lb.), blood meal (3 lb.), supplementary minerals and distilled water *ad lib.* have now been offered for periods of 2–3 wk. to four lactating cows, yielding 1–3 gallons per day, and though the rations supplied a total of only 2–3 gm. magnesium daily, the serum magnesium concentrations of the cows were little affected and remained within the range 2·2–2·7 mgm./100 ml.

To determine the effect of a yet more extreme reduction in magnesium intake, a single animal was transferred, over three days, from the above ration to a highly artificial one, of wheat straw (2 lb.), casein (3 lb.), starch (14 lb.) and supplementary minerals, which provided only 0·8 gm. magnesium daily. The full ration was consumed for two days only, but during this period the serum magnesium fell from 2·2 to 0·8 mgm./100 ml. and an equally rapid recovery in concentration occurred with a return to the original ration.

An intermediate ration, of wheat straw (1 lb.), paper pulp treated with liquid invert sugar (6 lb.), flaked maize (3 lb.), casein (2 lb.), starch (4 lb.) and supplementary minerals, supplying 1·5 gm. magnesium daily, has, with two cows to which it has been offered, proved acceptable and yet effective in causing rapid falls in serum magnesium concentration. The results obtained with one of these cows to which, in addition to the above ration, occasional supplements of magnesium were given are shown in Fig. 1.

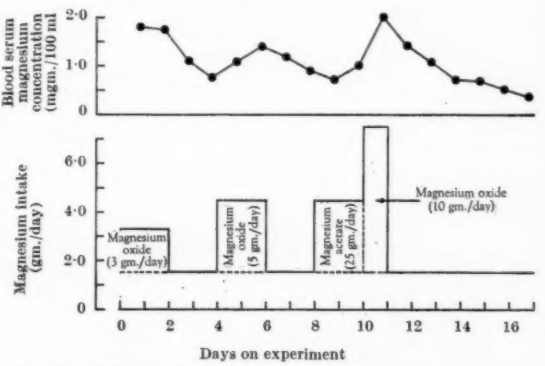

Fig. 1. Daily intakes of magnesium and the changes in the blood-serum magnesium concentration in a mature lactating cow given an artificial ration, providing 1·5 gm. magnesium daily, and occasional supplements of magnesium. (The supplement of magnesium acetate given on days 9 and 10 was not readily eaten, due to its bitter taste, and was therefore replaced by magnesium oxide. On day 17 the cow refused her food and was removed from experiment: the symptoms of tetany that can occur at a serum magnesium concentration of 0·7 mgm./100 ml. or less were not observed)

These results for a mature lactating cow show that though the net requirement for magnesium is low, the animal nevertheless depends on a continuous dietary supply for the maintenance of normal serum magnesium concentrations and at critically low levels of magnesium intake the serum magnesium concentration is rapidly affected by small changes in the intake of magnesium. They demonstrate that the freely available body reserves of magnesium must be small in relation to the requirements of a milking animal and give support to our contention[2] that hypomagnesæmia in cows at pasture is essentially nutritional in origin.

I am grateful to a number of colleagues, and in particular to Mr. V. W. Johnson, for help.

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Copper in Isopods

In crustaceans the hepatopancreas is the main organ of enzyme production. In isopods this organ consists of two types of cells which differ in size and were first distinguished by Weber[1]. This author observed in the smaller cells dense clumps of yellowish granules which became the subject of some controversy. Weber thought these granules to be 'enzymogens', a contention which was denied by Frenzel[2]. Patrick[3] held the granules to represent mucin or mucigens since they could be stained—so she claimed—only with mucin stains. Patané[4], on the other hand, found the granule in question to be untouched by mucin-specific histological techniques and he considered them as catabolic products since they gave reactions characteristic of purine bases.

So far the controversial granules have been observed in terrestrial isopods and in *Ligia oceanica*, a marine isopod which leads a semi-terrestrial life. The examination of some of these animals led me to the conclusion that the nature of the granules is quite different from that assumed by previous authors.

In an X-ray spectrophotometric investigation[5] it was found that terrestrial isopods contain enormous amounts of copper—as set out in Table 1.

Of the population with the highest copper content, *Porcellio scaber* from Cornwall, several specimens were dissected and their cuticles, intestines and hepatopancreas pooled. The analyses on these samples gave the values shown in Table 2.

Table 1

Species	Source	Copper (percentage of dry weight of whole animals) (range and mean)	No. of analyses
Porcellio scaber Ltr.	Suez, Egypt	0·022–0·039 (0·032)	5
Porcellio scaber Ltr.	Vienna, Austria	0·039–0·043 (0·041)	5
Porcellio scaber Ltr.	Cornwall	0·096–0·118 (0·106)	3
Armadillidium granulatum Br.	Spain	0·047–0·088 (0·063)	4
Armadillidium klugii Br.	Spalato, Yugoslavia	0·074–0·088	2
Armadillidium vulgare Latr.	Vienna	0·048–0·050	2
Cylisticus convexus B.L.	Vienna	0·051	1
Syspastus brevicornis Ebner	Elba	0·026–0·031 (0·028)	3

Table 2

Organ	Copper (percentage of dry weight)
Cuticle	< 0·005
Intestine + hepatopancreas	0·635
Intestine	0·076
Hepatopancreas	1·43

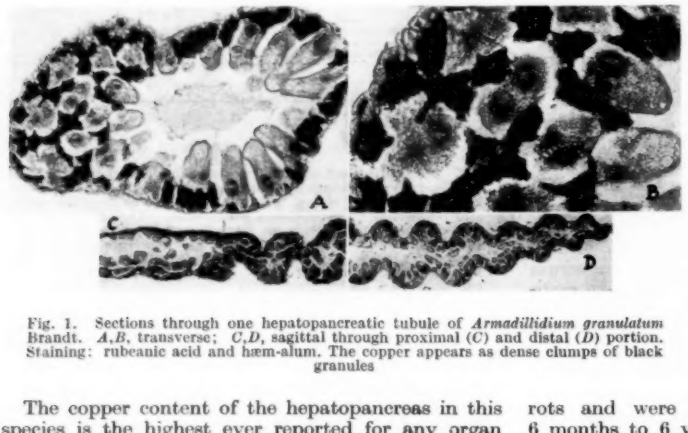

Fig. 1. Sections through one hepatopancreatic tubule of *Armadillidium granulatum* Brandt. *A,B*, transverse; *C,D*, sagittal through proximal (*C*) and distal (*D*) portion. Staining: rubeanic acid and hæm-alum. The copper appears as dense clumps of black granules

The copper content of the hepatopancreas in this species is the highest ever reported for any organ of an animal.

In sections of the hepatopancreas of *Armadillidium granulatum* stained with rubeanic acid (dithio-oximide)[6] the copper shows as dense black masses occurring intercellularly and in the small hepatopancreatic cells (Fig. 1). The large cells are free of copper which agrees with the findings of the earlier authors[1–4] that in the large cells granules do not occur. There can be scarcely any doubt that the granules observed by these authors are identical with, or contain, the copper made visible by staining with rubeanic acid.

The copper in the hepatopancreas of the animals investigated cannot be present as 'specific protein copper' since in such a state, as defined by Zuckerkandl[7], it would not react with a chelating agent like dithio-oximide. Moreover, the copper content of the hæmocyanin molecule, the cuproproteid most likely to occur in these animals, is only 0·18–0·22 per cent[8], thus even if the hepatopancreas consisted exclusively of hæmocyanin its copper content should be lower by almost one order of magnitude than it actually is. To account for its reaction with rubeanic acid the copper in the hepatopancreas of the isopods must be present in a reactive, easily dissociable state (the "cuivre naturellement réactive" of Zuckerkandl). How in this case toxicity is avoided and what the functions of these extraordinarily large amounts of copper could be remains to be investigated. Although up to now the occurrence of hæmocyanin in crustaceans has been proved only in stomatopods and decapods[9] it would not be unreasonable to expect the presence of this pigment in isopods, in which case part of the copper in the hepatopancreas might be in store for the synthesis of hæmocyanin.

All the isopods examined are reared on a diet of leaves and carrots and were kept in laboratory culture from 6 months to 6 years. Most of the isopods studied were collected and kept in culture by Prof. W. Kühnelt, whose co-operation in this investigation is gratefully acknowledged. The X-ray analyses were performed on mature, non-ovigerous, non-moulting animals in March 1961 by Dr. H. Makart at the Mineralogical Institute of the University, Vienna.

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'Quick' and 'Catch' Adductor Muscles in Brachiopods

THE purpose of this communication is to direct attention to the close analogy between lamellibranch molluscs and articulate brachiopods, in the structure and function of the adductor muscles. In view of the large body of work on the physiology of the adductors in lamellibranchs, it is probable that a similar study in brachiopods would yield interesting comparisons.

Like a lamellibranch shell, the shell of an articulate brachiopod consists of two valves hinged together; but brachiopod valves are placed dorsally and ventrally, are hinged on the posterior side, and have perfect bilateral symmetry. They are closed by two pairs of adductor muscles, attached separately to the dorsal valve but uniting into a single pair of tendonous attachments on the ventral valve; and they are opened not by a ligament as in lamellibranchs but by a pair of divaricator (= diductor) muscles, of which the lines of action pass behind the hinge axis (Fig. 1). The two pairs of adductors differ macroscopically in form and lustre. Microscopically, Hancock[1] noted more than a century ago that the fibres of the posterior adductors are striated and those of the anterior adductors unstriated. This observation has been doubted more recently[2], but Atkins[3] and I have been able to confirm it in several genera. The divaricators are unstriated. It has long been known that brachiopods, like lamellibranchs, snap their valves together for protection when disturbed and at other times for the ejection of pseudofæces. It has therefore seemed probable that the analogy extends further, and that the striated and unstriated adductors in brachiopods are, respectively, 'quick' and 'catch' muscles. During a recent examination of living brachiopods in New Zealand I was able to confirm this in the terebratelloids *Terebratella* (*Waltonia*) *inconspicua* (Sowerby), *T.* (*Magasella*) *sanguinea* (Leach) and *Neothyris lenticularis* (Deshayes), and in the rhynchonelloid *Tegulorhynchia* (*Notosaria*) *nigricans* (Sowerby).

When the valves snap together spontaneously, they do not close completely, and immediately afterwards begin to re-open. If the animal is disturbed (for example, by a vibration of the vessel in which it is contained), the valves snap rapidly until their edges are nearly together, and then close slowly until their edges are tightly in contact (the shell then remains closed for a variable period before re-opening). Thus both quick and slow closing movements can be distinguished in the undissected animal. If the tendonous portions of the anterior adductors are cut, leaving the posterior adductors undamaged, the valves snap together repeatedly but are unable to close tightly. If a posterior adductor is placed between a pair of electrodes connected to a stimulator, a single pulse will induce a single rapid twitch in the muscle. Thus the striated posterior adductors are clearly the muscles responsible for the rapid snapping of the valves. When an anterior part of the valve and mantle tissue is removed (making the muscles visible through the very thin anterior body-wall) the injury causes a permanent contraction of the anterior adductors, indicating that these are the muscles responsible for slow, tight and prolonged closure of the valves. The relatively slow reopening of the valves after a closure is due presumably to the contraction of the unstriated divaricators when the antagonistic contraction of the adductors has been relaxed.

The functional role of these muscular mechanisms will be described elsewhere, but it is hoped that the physiological properties of these muscles will be investigated more thoroughly by those with access to supplies of living brachiopods. This work was done at Portobello Marine Biological Station during the tenure of a Royal Society and Nuffield Foundation Commonwealth Bursary.

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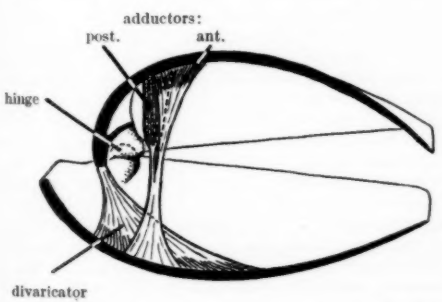

Fig. 1. *Terebratella inconspicua*, with valves gaping, cut along median plane of symmetry to show chief muscles (all other organs omitted); dorsal valve above. (× 3)

Paper Electrophoresis of Proteins while preserving the Pattern of their Localization in Tissues

THE composition of protein mixtures extracted from different sources can be found by the usual methods of paper electrophoresis, but the localization of protein fractions in tissues can only be determined with preliminary preparative breakdown of tissues and organs.

I have now succeeded in combining these two functions by using special multilayer columns for electrophoresis made by folding and compressing one or two large sheets of paper and inserting imprints on filter paper of leaves or thin slices of animal tissues or organs. Fig. 1 shows examples of leaf-imprints obtained by a method described in refs. 1 and 2.

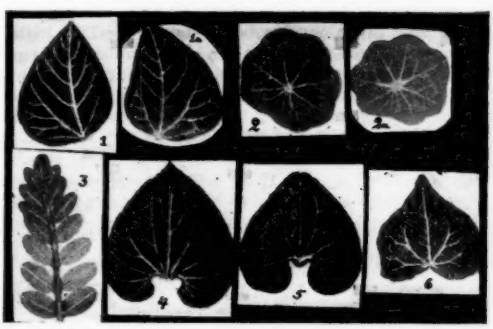

Fig. 1. Examples of leaf imprints on filter paper

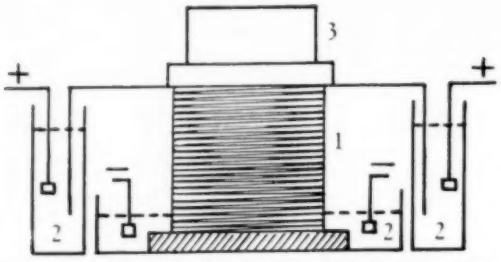

Fig. 2. Schematic diagram of electrophoresis on folded paper column. 1, Folded paper column; 2, buffer; 3, weight 1–2 kgm.

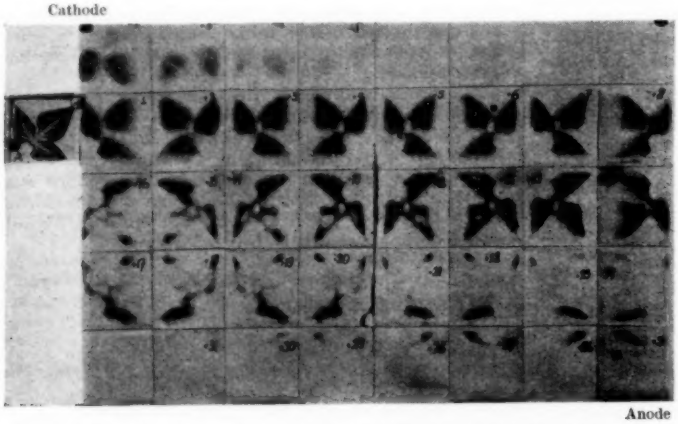

Fig. 3. An unfolded electrophoretogram of a bean leaf. Proteins are stained with bromophenol blue. Duration of electrophoresis was 2 hr. at 20 V./cm. The numbers 1, 2, 3, 4, *et seq.* indicate the sequence of layers in the folded column. *A*, the original leaf-imprint after electrophoresis. Chlorophylls and carotenoids did not migrate

Such imprints are inserted while still fresh and wet into a column of folded paper or a pile of paper disks, saturated with buffer and subjected to electrophoresis. The form of the leaf is preserved during migration of the proteins to the anode or cathode ; the localization of protein fractions may be determined by the usual methods of fixing and staining the unfolded paper electrophoretograms.

A schematic diagram of the electrophoresis is shown in Fig. 2. It is emphasized that better electrophoretograms are obtained by compressing the folded paper columns at 150–200 atm. before applying the buffer. Fig. 3 shows an electrophoretogram of a bean leaf, at the periphery of which is localized a rapidly migrating protein fraction.

This technique can probably be applied to histochemistry if materials with a finer microstructure than the usual filter paper can be used for impressing thin slices of tissues and organs. Folded, multilayer columns can of course also be used for electrophoresis of extracted mixtures of proteins.

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CYTOLOGY

Fixation of Yeast Protoplasts for Electron Microscopy

In an attempt to study the plasma membrane of cells by correlation of physico-chemical experiments and electron microscopical observations, yeast protoplasts were considered as a suitable experimental material. These protoplasts are easily obtained in quantities sufficient for chemical experiments by treatment of a yeast cell suspension with gut juice of *Helix pomatia*[1–3]. In the present work *Saccharomyces carlsbergensis* strain 74 of the National Collection of Yeast Cultures, Britain, has been used.

The protoplasts are osmotically fragile, but suspended in a solution containing 10 per cent mannitol and 0·01 *M* sodium acetate-acetic acid buffer they can be kept intact at 4° C. for some days. By phase-contrast microscopy they are seen as globular bodies with a diameter of 6–7μ containing a large vacuole of about 4μ diameter and some granular inclusions (Fig. 1). Addition of 1 per cent osmium tetroxide to such a suspension for fixation causes the sudden disappearance of the vacuole and a swelling of the protoplasts to complete disintegration. This is remarkable in view of the fact that other single cells, for example, *Limnaea* eggs, ascites tumour cells and blood cells are perfectly fixed by the addition of osmium tetroxide to their suspensions. A similar phenomenon, however, was observed by Thorsson and Weibull[4] and by Lee[5] with the fixation of bacterial spheroplasts.

It was assumed that the plasma membrane of the normal yeast protoplasts is permeable to water, but not, or with difficulty, to small molecules such as mannitol and to ions. Then it was supposed that the action of osmium tetroxide on the plasma membrane renders this membrane permeable to small molecules and ions but not to macromolecules. The same suppositions were made for the vacuole wall.

Of the substances contained in the cytoplasm the most interesting in the present case are the nucleic acids and the salts.

From the dimensions of protoplast and vacuole, combined with results obtained by Eddy[6] it can be calculated that the cytoplasm contains about 4·3 per cent ribonucleic acid and deoxyribonucleic acid together, that is, about 0·12 equiv. per l. A potassium content of the same order may be derived from a table given by Cook[7] for yeast cells, whereas the sodium and chloride content is much lower, namely, about 0·007 equiv. per l. The nucleic acids will be almost fully ionized at *p*H 6, the *p*H of the cytoplasm.

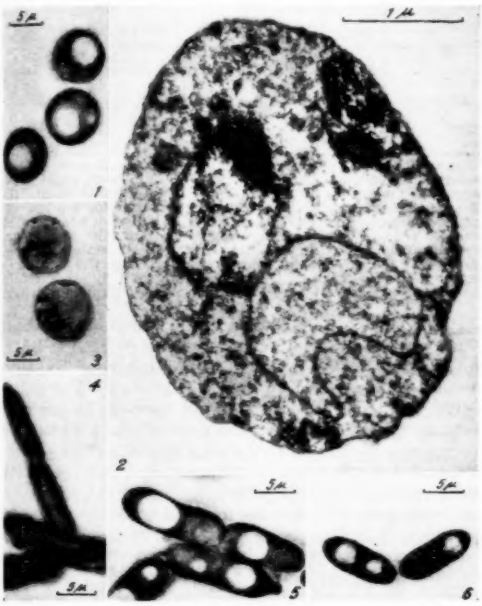
From the Donnan equilibrium, set up under the conditions which are thought to prevail after fixation, the resulting swelling pressure of the cytoplasm can be derived. This pressure is represented by the difference between the concentrations of the mobile ions inside and outside the cytoplasm. In a 10 per cent cell suspension this difference will amount to about 0·1 molar. The action of this swelling pressure leads to disintegration of the protoplast.

It proved to be impossible to counteract this swelling pressure, which makes its appearance only at fixation, by the extra addition of uncharged molecules, for example, mannitol or sucrose, to the fixation medium in order to raise its osmotic pressure.

This is in agreement with the picture given here. The effect of the swelling pressure on the protoplast will be prevented in a mechanical way by the toughness of a cell wall around the cytoplasm. In fact this is the case with normal yeast cells, which can be fixed by osmium tetroxide without disruption of the cell. The protoplasts, without such a wall, may be protected at fixation by an inversely directed Donnan effect, resulting from the addition of highly charged, non-permeating, macromolecules to the fixation fluid.

It has been found that carrageen, a sulphate colloid of equivalent weight 294, yields good results in this respect.

Fig. 2 shows an electron micrograph of a yeast protoplast fixed by osmium tetroxide at *p*H 5 in a solution containing 0·6 per cent calcium carragenate.



Figs. 1–6

As regards the fate of the vacuole the same line of explanation may be followed. The vacuole wall will be rendered permeable to small molecules and ions by the action of osmium tetroxide. The vacuole then collapses as a result of the swelling tendency of the cytoplasm and it does so in protoplasts as well as in normal yeast cells (Figs. 3 and 4). Fig. 5 shows unfixed normal yeast cells. As it is obviously impossible to prevent the uptake of solute by the cytoplasm out of the vacuole by the addition of highly charged macromolecules to the vacuole content, there is no issue unless the cause of the Donnan effect itself is abolished. This might be done by lowering the *p*H of the fixation medium to the isoelectric point of nucleic acid, assuming that the same *p*H will be reached in the cytoplasm.

Indeed, with fixation at *p*H 1·45 the vacuole of normal yeast cells does not collapse (Fig. 6).

With yeast protoplasts the picture is less clear. There is some shrinkage of the vacuole, but this may be due to an insufficient difference in diffusion-rate between osmium tetroxide and hydrochloric acid. Evidently the osmium tetroxide should arrive at the vacuole wall after the acidification of the cytoplasm, for a vacuole once collapsed will not swell again. It is feasible that the presence of a cell wall may retard the diffusion of osmium tetroxide sufficiently to this end.

The bearing of the present results on the quality of fixation of other macromolecular polyelectrolyte containing cells or isolated cell constituents seems worthy to be investigated. A more detailed report of the experiments will be submitted for publication elsewhere.

I wish to thank Dr. G. J. H. Th. Tonio for the generous supply of yeast protoplasts and Prof. J. Th. G. Overbeek for a discussion of the results.

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VIROLOGY

Influence of *p*-Fluorophenylalanine on the Production of Viral Ribonucleic Acid and on the Utilizability of Viral Protein during Multiplication of Fowl Plague Virus

Experiments by Zimmermann and Schäfer[1] showed that *p*-fluorophenylalanine (FPA), given immediately after infection of chick embryo cells by fowl plague virus, inhibits the formation of any virus material detectable by commonly used biological methods. If FPA is given 2 hr. post-infection, the *s*-antigen appears normally, while no hæmagglutinin activity and no infectious particles can be detected.

With the aid of isotopic techniques two questions were explored. (1) How can the synthesis of viral ribonucleic acid (RNA) be influenced by FPA? (2) Does the *s*-antigen, which is synthesized in the presence of FPA, fit into the viral particle?

In a recent paper a method was described by which virus-specific ribonucleic acid (RNA) can be demonstrated within tissue culture cells infected with fowl plague virus[2]. The following technique was used. Phosphorus-32 was added to the culture medium. In this way only the newly synthesized RNA was labelled so that the bulk of cell-RNA could be excluded. To compare the RNA of the infected cultures with that of the non-infected ones, the RNA's were digested with RNase and the specific nucleotides and oligonucleotides were determined quantitatively. The ratios of cytidine monophosphate to oligonucleotides are characteristic for each species of RNA[3]. By the shift of the oligonucleotide pattern of the infected cells towards the pattern of purified viral RNA, one can estimate the amount of virus-RNA in the mixture with the newly built cell RNA.

In preliminary experiments it was found that the overall incorporation of phosphorus-32 into the cell-

Table 1. Effect of FPA on the Synthesis of Viral RNA

Exp.	C/X	FPA 0		FPA 2		no FPA		Virus
		contr.	vir.	contr.	vir.	contr.	vir.	RNS
1	U	1·25	1·22	1·24	1·10	1·23	1·08	0·86
	AC	2·97	2·99	3·01	2·62	2·96	2·68	2·60
	GC	1·83	1·85	1·83	1·96	1·84	2·04	2·08
2	U	1·44	1·44	1·43	1·28	1·48	1·28	0·86
	AC	3·29	3·31	3·47	2·98	3·34	2·66	2·60
	GC	1·81	1·89	1·87	1·99	1·84	1·87	2·08
3	U	1·18	1·17					
	AC	2·90	2·92					
	GC	1·80	1·92					
4	U			1·66	1·51	1·70	1·50	0·86
	AC			3·33	2·96	3·75	2·88	2·60
	GC			1·82	1·90	1·89	1·82	2·08

For each sample five cultures of 24-hr.-old chick embryo cells were taken. To one group immediately after infection 270 γ/ml. FPA were added (FPA 0 vir.), to another group FPA was added 2 hr. post-infection (FPA 2 vir.). The third group received no FPA (no FPA vir.). All control cells (contr.) were not infected. In experiment 1 the cultures received 0·15 mc. phosphorus-32/culture 2 hr. post infection and were collected at 5 hr. post infection. In experiments 2 and 4, the cultures received the isotope at 4 hr. post infection, in experiment 3 at 1 hr. post infection. They were collected 2 hr. later, respectively. Only four nucleotides were determined. The ratios o cytidinemonophosphate (C) to nucleotide (X) are shown.

RNA was not affected by FPA. Particular interest was directed to the problem of whether viral RNA can be synthesized under conditions where *s*-antigen production has been stopped by FPA. If FPA is given immediately after infection, there is no difference between the RNA's of infected and non-infected cells (Table 1). Under these conditions *s*-antigen production is completely suppressed. If, however, FPA is given 2 hr. post-infection, the oligonucleotide pattern of the RNA of infected cells shifts towards that of the purified virus (compared with non-infected cells), just as in experiments in which no FPA was given.

Since FPA inhibits the synthesis of viral RNA only at early stages after infection, one may assume the formation of some kind of an 'early protein', resembling that of phages[4–6] necessary for the start of specific nucleic acid synthesis. If this hypothetical 'early protein', which is apparently not identical with the *s*-antigen, incorporates FPA instead of phenylalanine it will not be functional and synthesis of viral RNA cannot start. Once it has been synthesized normally, FPA no longer interferes with the production of either viral RNA or the material behaving serologically like *s*-antigen. Since fowl plague RNA does not contain an abnormal nucleotide[7], the 'early protein' may not be a particular enzyme, because all enzymes necessary for RNA-synthesis are already present in the non-infected cell. Its function may be to stabilize somehow the genetic information of the virus.

The assumption of a faulty 'early protein' raises the question of whether the *s*-antigen formed in the presence of FPA is identical in all respects with that produced without FPA during multiplication.

In preliminary experiments it was found that FPA had no marked influence on the incorporation of ^{14}C-leucine into normal cell proteins.

The treatment of tissue cultures with FPA was the same as that in the experiments with phosphorus-32. All cultures received ^{14}C-leucine during 2–5 hr. post infection. The incorporation of isotope was stopped by addition of large amounts of ^{12}C-leucine and virus production proceeded for another 17 hr. after removal of the FPA-block 5 hr. post-infection by phenylalanine. The virus contained in the medium was purified after addition of non-radioactive-carrier virus by adsorption on and elution from erythrocytes and was then split by ether[8]. After centrifuging to remove unsplit virus the hæmagglutinin was taken off by erythrocytes. All purification steps were followed by the determination of the biological activities in order to assure that no losses in any of the samples occurred. In each experiment all samples were treated identically.

Control cultures were grown on a ^{14}C-leucine medium prior to infection and after removal of the isotope the cells were infected. This virus yield was treated in the same way as, and compared with, virus grown at the same time in the presence of ^{14}C-leucine. The *s*-antigen of the control virus contained only 10–15 per cent of the radioactivity of that grown in the presence of ^{14}C-leucine. This radioactivity probably represents normal cell components, which cannot be separated from the virus under these conditions.

Table 2. Effect of FPA on the Incorporation of *s*-Antigen into Intact Virus

Exp.	Sample	C.p.m. in *s*-antigen	Virus yield in culture fluid HA-titre
1	FPA 0	753	128
	FPA 2	750	128
	Virus	10,000	256
2	FPA 0	211	32
	FPA 2	208	32
	Virus	2,315	64
3	FPA 2	429	128
	Virus	5,500	256

For each sample 10 cultures of chick embryo cells were infected synchronously by fowl plague virus. 270 γ/ml. FPA were added immediately after infection (FPA 0) or 2 hr. post-infection (FPA 2). The virus samples received no FPA. At 2 hr. post-infection 0·5 μM ^{14}C-leucine/culture (specific activity 6·24 mc./mM) were added to all cultures. The medium was withdrawn 5 hr. post-infection and medium containing 600 γ/ml. phenylalanine and ^{12}C-leucine 300 times in excess was added. After 17 hr. the virus was isolated from the medium by adsorption on and elution from red cells, and split by ether (ref. 8). The radioactivity was determined in the isolated *s*-antigen fraction.

The experiments shown in Table 2 demonstrate that *s*-antigen, synthesized in the presence of FPA (FPA 2) and labelled by ^{14}C-leucine, is not incorporated into the intact virus in an appreciable amount. The radioactivity in this fraction does not surpass 15 per cent of that of virus grown without FPA. The same percentage of radioactivity was found in the above-mentioned controls and in experiments in which FPA was given immediately after infection (FPA 0). Under these conditions no serologically detectable *s*-antigen is produced in the presence of ^{14}C-leucine.

It is known that FPA is incorporated into proteins instead of phenylalanine[9]. From our results we conclude that the faulty *s*-antigen, which incorporated FPA, is still serologically demonstrable but does not fit into the intact virus and therefore is excluded from incorporation into it as a viral sub-unit.

We wish to thank Prof. W. Schäfer for his interest in these experiments. This work was supported by the Deutsche Forschungsgemeinschaft.

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FORTHCOMING EVENTS

Sunday, September 3—Wednesday, September 6

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE (at Norwich)—Continuation of 123rd Annual Meeting.

Sunday, September 3

At 11 a.m.—Service in Norwich Cathedral. Preacher: The Lord Bishop of Norwich, the Right Reverend Dr. Launcelot Fleming.

Monday, September 4

At 10 a.m.—Prof. C. F. Carter: "The Economic Use of Brains" (Presidential Address, Section F).

At 10 a.m.—Dr. K. P. Oakley: "Dating Man's Emergence" (Presidential Address, Section H).

At 11.30 a.m.—Dr. Richard G. Lipsey: "Can there be a Valid Theory of Wages?—A Study of Fact and Fiction in Economics" (Lister Lecture).

At 2.30 p.m.—Dr. Judith Milledge: "Form and Colour in Nature" (Darwin Lecture).

At 8 p.m.—Dr. B. V. Bowden: "The Impact of Automation" (Evening Discourse).

Tuesday, September 5

At 8 p.m.—Mr. Kenneth Adams: "Seeing in the Round" (*Eidophor* programme).

Sunday, September 3—Thursday, September 14

ROYAL AERONAUTICAL SOCIETY; INSTITUTE OF AEROSPACE SCIENCES, United States; and the CANADIAN AERONAUTICAL INSTITUTE (at the Royal Aeronautical Society, 4 Hamilton Place, London, W.1)—Eighth Anglo-American Aeronautical Conference.

Wednesday, September 6—Saturday, September 16

INTERNATIONAL FEDERATION FOR DOCUMENTATION (in the Caxton Hall, Westminster, London, S.W.1)—27th Conference.

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

LECTURER or ASSISTANT LECTURER IN PSYCHOLOGY at Queen's College, Dundee—Secretary of the University, University of St. Andrews, c/o Queen's College, Dundee (September 7).

RESEARCH ASSISTANT (graduate in science, preferably physics, with a knowledge of electronics) IN THE DEPARTMENT OF AGRICULTURE, for investigations into the measurement of radioactivity in livestock—The Registrar, The University, Leeds 2 (September 7).

LECTURER (with a degree or equivalent qualification, and teaching or other suitable experience) TO TEACH PHYSICS AND MATHEMATICS for Higher National Diploma Courses—The Principal, Royal Radar Establishment College of Electronics, Malvern (September 9).

ASSISTANT LECTURER IN OCCUPATIONAL PSYCHOLOGY—The Secretary, Birkbeck College (University of London), Malet Street, London, W.C.1 (September 11).

SENIOR LECTURER (graduate in bacteriology or dairying with considerable experience of agricultural and/or dairy bacteriology) IN BACTERIOLOGY—The Secretary, West of Scotland Agricultural College, 6 Blythswood Square, Glasgow, C.2 (September 11).

SCIENTIFIC OFFICERS or SENIOR SCIENTIFIC OFFICERS (with a first- or second-class honours degree, preferably in science or engineering, with a wide and intelligent interest in scientific matters and aptitude for administrative work in a scientific field) in the D.S.I.R. Headquarters, London, for scientific administrative work—Establishment Office, Department of Scientific and Industrial Research, 5–11 Regent Street, London, S.W.1, quoting HQ.4 (September 16).

TECHNICAL OFFICER (preferably with experience in a teaching laboratory) IN THE BIOCHEMISTRY DEPARTMENT, University of Melbourne, Australia, for the organization at technical level of undergraduate practical classes—The Secretary, Association of Universities of the British Commonwealth, Marlborough House, Pall Mall, London, S.W.1 (September 16).

LECTURER or ASSISTANT LECTURER IN PURE MATHEMATICS—The Secretary, The University, Exeter (September 18).

CURATOR (with a B.Sc. degree with particular training in mineralogy and palæontology) OF FOSSILS AND MINERALS in the Natural History Museum, Adelaide, South Australia—The Agent-General for South Australia, 50 Strand, London, W.C.2 (September 22).

ECONOMISTS (2) (with a first- or second-class honours degree in economics and at least three years postgraduate experience in industry or research in industrial economics; preferably with a lively interest in technological development) in the D.S.I.R. Headquarters, Economic Section, London, to take part in planning and executing studies of the research and development needs of industries or sectors of the national economy—The Manager (PE.3324), Ministry of Labour, Professional and Executive Register, Atlantic House, Farringdon Street, London, E.C.4 (September 25).

SCIENTIFIC ASSISTANT (preferably with experience in plant nematology) to undertake the identification of helminthological collections—The Director, Commonwealth Bureau of Helminthology, The White House, 103 St. Peter's Street, St. Albans, Herts (September 30).

CHEMIST (with a degree in chemistry from an approved university, together with at least two years postgraduate experience, and a working knowledge of experimental statistics), to be responsible for a routine soil testing laboratory and to conduct research on tea soils and on the nutrition of the tea bush—The Director, Tea Research Institute of East Africa, P.O. Box 91, Kericho, Kenya Colony (October 1).

LECTURER (preferably with special qualifications in physiology or genetics) IN BOTANY; and a RESEARCH FELLOW (with a degree, preferably some research experience, and between the ages of 23 and 35 years) IN BOTANY at the University of Tasmania, Australia—The Secretary, Association of Universities of the British Commonwealth (Branch Office), Marlborough House, Pall Mall, London, S.W.1 (Australia and London, October 2).

LECTURERS IN THE DEPARTMENT OF MEDICAL MICROBIOLOGY, Makerere University College, East Africa—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1 (October 4).

CHAIR OF CHEMICAL PATHOLOGY—The Secretary, The University, Aberdeen (October 31).

POSTDOCTORAL RESEARCH FELLOW IN THE CHEMISTRY DEPARTMENT, for physico-chemical investigations of the surface chemistry of organic polymers of interest to the tyre industry—The Dean of Graduate Studies, University of Melbourne, Parkville, N.2, Victoria, Australia (November 30).

ASSISTANT BIOCHEMIST, Basic grade (preferably holding the A.R.I.C., the Grad.Mem.R.I.C., or B.Sc.(Hons.) in chemistry or biochemistry), in the Group Laboratory at Crumpsall Hospital—The Hospital Administrator, Crumpsall Hospital, Manchester 8.

ASSISTANT LECTURER, Grade B (with a good honours degree in mathematics and preferably some teaching or research experience) IN MATHEMATICS—The Clerk to the Governors, South-East Essex Technical College, Longbridge Road, Dagenham, Essex.

ASSISTANT PROFESSOR OF BIOLOGY—Head, Department of Biology, University of New Brunswick, Fredericton, N.B., Canada.

ASSISTANT PROFESSOR OF PHYSIOLOGY—The Dean of Medicine, University of Alberta, Edmonton, Alberta, Canada.

BIOCHEMIST (with a good honours degree and preferably post-graduate training or research experience, and interested in the use of advanced techniques, for example, high voltage electrophoresis, and in the invention of new techniques for the differentiation of bacteria), for duties which will include the supervision of a survey now in progress—The Secretary, National Institute for Research in Dairying, Shinfield, Reading, quoting Ref. 61/26.

BIOMETRICIAN (with a first- or upper second-class honours degree, together with an interest in biological research, and preferably experience in agricultural experimentation) IN THE DEPARTMENT OF BIOMETRICS—The Secretary, Grassland Research Institute, Hurley, Maidenhead, Berks.

D.S.I.R. RESEARCH ASSISTANT (graduate) IN ZOOLOGY, to study the responses to environmental stress of the reticulo-endothelial system of the spleen and liver in rodents, using histochemical and enzymological techniques—Dr. L. L. de Kock, Department of Zoology, The University, Aberdeen.

GEOLOGIST (under 35, with a first- or good second-class honours degree in geology, and preferably some background of economic geology and/or a knowledge of mapping, photogeological interpretation and surveying with theodolite and aneroid barometers) in the British Solomon Islands, for geological field surveys, studies of mineral occurrences, and petrological or palæontological studies—The Director of Recruitment, Department of Technical Co-operation, Carlton House Terrace, London, S.W.1, quoting C1/RC218/18/01.

GRADUATE CHEMIST IN THE SOIL SCIENCE LABORATORY, University of Oxford, to assist research into the use of new physico-chemical techniques for assessing the availability to plants of potassium in the soil—Dr. P. H. T. Beckett, Department of Agriculture, Oxford.

LABORATORY TECHNOLOGIST (A.I.M.L.T., and considerable experience, preferably as Chief Technician) with the Government of Tanganyika—The Crown Agents, 4 Millbank, London, S.W.1, quoting Ref. M3C/53605/NF.

LECTURER or ASSISTANT LECTURER IN AGRICULTURAL ECONOMICS; and a LECTURER OR ASSISTANT LECTURER IN FARM MANAGEMENT in the Faculty of Agriculture (Imperial College of Tropical Agriculture), University College of The West Indies, Trinidad, West Indies—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1.

LECTURER (with a good honours degree and capable of teaching physiology and pharmacology up to and including the external honours B.Pharm. degree of the University of London) IN PHYSIOLOGY AND PHARMACOLOGY in the Department of Biology—The Registrar, Leicester College of Technology and Commerce, Leicester.

LECTURER (under 45, with either A.M.I.E.E. or Higher National Diploma in Electrical Engineering, and at least three years teaching experience or relevant industrial experience), for an electrical technician's course at Kampala Technical Institute, Uganda—The Director of Recruitment, Department of Technical Co-operation, Carlton House Terrace, London, S.W.1, quoting BCD.114/9/032/T.

LIBRARIAN (woman zoologist, age 25–35, British nationality, with a degree in zoology or biology) in an ecological library, for duties which include subject-indexing—The Director, Bureau of Animal Population, The University, Botanic Garden, Oxford.

PHYSICIST (with a first- or second-class honours degree) IN THE MEDICAL PHYSICS UNIT, to apply physics to a wide range of medical problems, initially in the radioactive isotope field—The Secretary, S.E. Regional Hospital Board, 11 Drumsheugh Gardens, Edinburgh 3.

RESEARCH ASSISTANT (honours graduate) IN PHYSICS—The Clerk to the Governors, Woolwich Polytechnic, London, S.E.18.

RESEARCH ASSISTANT (with a good degree in zoology and an interest in the physical and chemical properties of animal tissues) IN THE STUDY OF MOLECULAR STRUCTURE OF BIOLOGICAL SYSTEMS in the Department of Bimolecular Structure—The Registrar, The University, Leeds 2.

SCIENTIFIC OFFICER (with a first- or second-class honours degree in chemistry or Associate or Fellow of the Royal Institute of Chemistry, and preferably postgraduate experience in analytical work in food chemistry and insecticide analysis) in the Ministry of Commerce and Industry, Nigeria, for chemical work in connexion with the deterioration and protection of raw foodstuffs during storage in Nigeria—The Recruitment Attaché, Nigeria High Commission, 9 Northumberland Avenue, London, W.C.2, quoting Ref. E.10/2.

XUM

SENIOR LABORATORY TECHNICIAN or LABORATORY TECHNICIAN (experienced in bacteriology)—The Director, Laboratory Animals Centre, M.R.C. Laboratories, Woodmansterne Road, Carshalton, Surrey.

SENIOR LECTURERS or LECTURERS (with a first- or second-class honours degree or equivalent and preferably with teaching and/or research experience) IN MATHEMATICS, ELECTRONICS AND ENGINEERING PHYSICS—The Registrar, Royal Military College of Science, Shrivenham, Swindon, Wilts, quoting Ref. HQ.77/17.

SENIOR RESEARCH ASSISTANT (Ph.D. standard) IN PHYSICS, for a programme of work on the magnetic properties of thin films—The Principal, Royal Holloway College (University of London), Englefield Green, Surrey.

SENIOR RESEARCH FELLOW (with a university degree in science or engineering and experience in research) IN THE DEPARTMENT OF AIRCRAFT DESIGN, to engage in research in the fields of aircraft structure theory and/or solid mechanics—The Recorder, The College of Aeronautics, Cranfield, Bletchley, Bucks.

STUDENT TECHNICIAN IN THE DEPARTMENT OF PATHOLOGY, Connaught Hospital, Walthamstow, London, E.17—Secretary, Forest Group H.M.C., Langthorne Road, London, E.11.

STUDENT TECHNICIAN or TECHNICIAN (preferably with mathematics and physics at advanced level), for research work in a pulmonary physiology laboratory—The Secretary, University College Hospital Medical School, London, W.C.1.

TECHNICIAN (male or female) IN THE MICROBIOLOGICAL TEACHING LABORATORY—Mr. Edwards, The School of Pharmacy, 29–39 Brunswick Square, London, W.C.1.

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